SCIENCE

FRIDAY, MAY 10, 1912

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE FUNGUS OF THE CHESTNUT-TREE BLIGHT[1]

THE disease of chestnut trees now causing serious injury in the eastern states was first noticed about the year 1904 in the vicinity of New York and is believed to be due to the growth of a hitherto unknown fungus described by Murrill in 1906 under the name of *Diaporthe parasitica*. The question naturally arises: Is the *Diaporthe* a native species of this country which had escaped the observation of mycologists, or is it an introduced exotic species? If it is a native species we must next ask how it happens that so severe an epidemic has appeared suddenly, for, if the fungus has always existed here, the important practical question is: What temporary conditions have arisen which have enabled it to increase at a rate previously unknown? Although there are some vague references to diseased chestnut trees in agricultural journals published a good many years ago, there is nothing in them to warrant us in believing that a disease at all comparable with the present chestnut epidemic ever prevailed here. One thing, at least, is certain. Had there been within the last fifty or sixty years a disease of chestnuts in the region now devastated similar to the present epidemic, there would have been left no large trees to be attacked now.

In general it may be said that when a disease due to a fungus appears suddenly and with great severity, the chances are that it is owing to the introduction of some exotic species which, although it may not

[1] Paper read April 19, 1912, before the American Philosophical Society, Philadelphia.

cause serious trouble in the country from which it came, is able to produce disastrous effects on our native species which have not through long exposure become more or less immune to the fungus. It has been suggested that the chestnut-tree fungus was introduced from Japan. The arguments advanced by those who adopt this view do not seem to me to be at all conclusive. That the *Diaporthe parasitica* is a native of Japan remains to be proved. Furthermore, we have no accounts of any disease of chestnuts in Japan similar to our present disease. If I am not mistaken, the main reason for thinking that the disease might have come from Japan was the statement which had been made that Japanese chestnuts grown in this country did not contract the disease. That they are really immune is, to say the least, very doubtful, and is positively denied by some experimenters.

The first fruiting specimens of the chestnut-blight fungus which I was able to examine struck me as having a close resemblance externally to what is generally known in American herbaria as *Endothia gyrosa* and also to a specimen issued in an Italian series of fungi exsiccati. With regard to the North American specimens of *Endothia* I shall speak later. The Italian specimen to which I refer is No. 986 of the first series of the *Erbario Crittogamico Italiano* issued in 1863. The label states that the fungus grew on chestnut trunks at Locarno on Lake Maggiore, where it was collected by Daldini in 1862. The name there given is *Endothia radicalis,* on which more needs to be said in connection with American specimens. The Italian specimen referred to has ascospores which seen to me to be the same as those of American specimens of *Diaporthe parasitica,* and my opinion is shared by some other mycologists who have examined the specimens in question. Of European botanists who have expressed the opinion that *Diaporthe parasitica* and *Endothia gyrosa* are identical may be mentioned Von Höhnel and Saccardo.

But the Italian fungus, by whatever name we call it, is not known to cause a disease of chestnuts in Italy, where, in consequence of the commercial value of the chestnut, the fungi which attack it have been carefully studied. Several diseases of chestnuts, due to fungi, are known in Italy, but the fungi which cause them are not any form of *Endothia.* In spite of the fact that the *Endothia* does not cause a recognized disease in Italy, it is conceivable that, if introduced into this country, it might cause serious damage to American species of chestnuts, since they have not by long exposure to the fungus become immune.

In this connection it should be stated that the Italian chestnut trees cultivated in this country are said to be attacked and destroyed by *Diaporthe parasitica* as well as our native species of chestnuts. We have also some recent experiments of Pantanelli, who in the *Rendiconti Accademia dei Lincei* of 1911 gave an account of inoculations made at Rome with spores of *Diaporthe parasitica* received from America. He made three sets of experiments. In one he inoculated sterilized dead branches of the Italian chestnut; in another living branches kept in closed cultures, and in the third he inoculated small chestnut trees placed in dishes in his laboratory. From the first two series of experiments, although the spores of the American material germinated, and developed a mycelium and conidia, we can infer only that the fungus in closed cultures may be made to grow as a saprophyte on the Italian chestnut, but we can infer nothing as to its parasitic action. In the third series of

experiments Pantanelli inoculated sixteen plants. Of these seven dried up, whether from the action of the fungus or from some other cause is not quite clear. From the other cases Pantanelli concludes that the American fungus may cause serious damage to Italian as well as American chestnuts. No control plants appear to have been used. To obtain a clear idea of the action of the fungus a more detailed account is to be desired.

Pantanelli compares the American *Diaporthe parasitica* with the five other species of *Diaporthe* which have been recorded on chestnuts and he considers it different from any of them, in which opinion he is correct. The important point, however, is not to distinguish between *Diaporthe parasitica* and the other five species of that genus, but between *Diaporthe parasitica* of America and *Endothia radicales* of northern Italy, to which it is evidently more closely related than to any of the other *Diaporthe* species. It is also to be desired that series of experiments with the inoculation of the spores of the Italian *Endothia* on Italian and American species of chestnut be made.

Before proceeding farther let me recapitulate what has already been said. First, our chestnut-blight fungus, if an imported species, is not likely to have come from Japan. Secondly, a fungus noticed on chestnuts in Italy as long ago as 1862 in external appearance and the microscopic characters of the perithecia, asci and spores so closely resembles the American chestnut fungus that they have been considered identical by some well-known European botanists. Thirdly, the American fungus is believed to be the cause of a very serious disease of American chestnuts and also to attack Italian chestnuts grown in America, while, on the other hand, the Italian fungus does not produce any clearly recognized disease.

If we now turn to the question whether the fungus of the present chestnut blight can be considered identical with any species previously known in America, we find ourselves involved in a maze of conflicting descriptive and bibliographical details which must utterly confuse those who are not expert mycological systematists, and even experts may be pardoned if they hesitate to express a very decided opinion on the subject. Although it can not be expected that any but specialists would be interested in the study of the very scattered literature relating to the subject, it may be of interest to others to have a general statement as to why it is so confusing even to experts.

As has been said, the name on the label of the specimen in the Erbario Crittogamico Italiano is *Endothia radicalis,* which to mycologists signifies that the Italian botanists, Cesati and De Notaris, to whom the naming of the specimen is to be attributed, were of the opinion that the Italian fungus was not a new species, but was identical with *Sphæria radicalis* of Schweinitz described in his "North American Fungi" in 1832, which they erroneously quote as the species on which Fries founded the genus *Endothia* in 1849. The genus was really founded on *Sphæria gyrosa* of Schweinitz from North Carolina, described still earlier, in 1822. Subsequent writers, however, considered *S. gyrosa* and *S. radicalis* as the same species to which the earlier specific name *gyrosa* should be given. Besides the specimen issued in the *Erbario Crittogamico* other Italian specimens were distributed in Rabenhorst's "Herbarium Mycologicum," Thuemen's "Mycotheca Universalis" and Saccardo's "Mycotheca Veneta," and as early as 1829, only seven years after Schweinitz's original description, *Sphæria gyrosa* was reported in Italy by Rudolphi in *Linnæa.*

Since 1829 there have been numerous references to the same fungus occurring in other European countries. In 1830 Fries stated in *Linnæa* that it had been found in France, and Tulasne in his "Carpologia," 1863, refers in detail to French specimens. In 1870 Fuckel and in 1886 Winter referred to its occurrence in Germany and specimens were distributed by Roumeguère in "Fungi Gallici" and Portuguese specimens in "'Fungi Lusitanici." In the recent "Flora Italica Cryptogama," 1906, the species is said by Traverso to occur in still more remote regions. If we are to trust the writers above mentioned *Endothia gyrosa*, originally described from North Carolina, is a species which is widely scattered through the northern hemisphere. It is generally said to grow on dead wood and in no cases is there any mention of a serious disease of the trees attacked. The hosts mentioned are, besides *Castanea, Æsculus, Alnus, Carpinus, Corylus, Fagus, Juglans* and *Quercus.*

Although, assuming that *Endothia gyrosa* and *Endothia radicalis* are only different names for a single species, all the European mycologists mentioned agree in believing that their *Endothia* on chestnuts is identical with the *Endothia gyrosa* of North America, we must ask ourselves whether their opinion is correct. This brings us to the main question, or rather conundrum: What is *Endothia gyrosa?* If we could answer that question most of the systematic difficulties which perplex us would disappear. Unfortunately, it seems to be almost impossible to be sure of what Schweinitz included under his *Sphæria gyrosa.* Specimens are in the Schweinitzian Herbarium in the Academy of Natural Sciences in Philadelphia, and other specimens of Schweinitz are to be found in a number of other herbaria in this country and Europe. Through the kindness of Professor Stewardson Brown I have been able to examine the specimens in the academy's collection and I have also examined Schweinitzian specimens in some other herbaria and have obtained information from others who have examined specimens which I have not seen. All the specimens I have seen agree in external appearance, the fungus looking to the naked eye like brownish-orange, pustulate cushions usually growing in cracks in the bark. Unfortunately, microscopic examination does not show ascospores, which are necessary to distinguish the species with accuracy. In American herbaria one sees many specimens marked *Endothia gyrosa* by different collectors, but almost always what was said of Schweinitzian specimens applies to these, viz., they show no ascospores. The same is true of the specimens distributed in European exsiccati with the exception of the No. 986 of the *Erbario Crittogamico.* The problem is to find undoubted Schweinitzian specimens with ascospores and here one must be careful to distinguish between what may be and what certainly are Schweinitzian specimens. I have not finished my search, but from my experience up to the present time it looks as if it were doubtful whether good Schweinitzian specimens with ascospores can be found. Others, however, may be more successful, but since Schweinitz himself did not make use of microscopic characters it is hardly worth while to spend much more time in discussing what he understood by *Sphæria gyrosa,* since it is now known that there are at least two species in this country which in gross appearance resemble *Sphæria gyrosa,* but which differ in the size and shape of the ascospores.

Unless we can obtain more information than has yet been possible, it will be better to consider that the authority for the genus *Endothia* should be Fries, emended by

Cesati and De Notaris, who in their "Sferiacei Italici," 1863, gave a sufficiently detailed and accurate description of *Endothia radicalis* with a recognizable figure, so that, taken in connection with the specimen in the *Erbario Crittogamico,* there can be no doubt as to what they understood by the species. This may or may not be the same as the *Sphæria radicalis* of Schweinitz, but certainly no genuine Schweinitzian specimens which I have ever seen would warrant any one in expressing a definite opinion. As far as one can distinguish species by their morphological, apart from their pathogenic, characters, *Diaporthe parasitica* seems to me to resemble the Italian *Endothia radicalis* so closely that they can not be separated specifically unless it be by some peculiarity not hitherto recorded.

There is still another point which should be considered. Is the fungus of our chestnut blight ever found on other trees? I have received a series of interesting specimens collected by Professor G. P. Clinton, which will illustrate this point. In some the bark of chestnuts and in others the bark of oaks is infested with an *Endothia* which in general appearance and in microscopic structure seem to me to be the same species. It is not, however, true that all the *Endothiæ* which occur on oaks belong to this species. There is an *Endothia* which appears to be common on oaks in the south, especially Florida and Louisiana, of which I collected material myself in New Orleans, which is clearly distinct from the *Endothia* of chestnuts, having ascospores much narrower and of a shape more nearly linear or bacilloid. Schweinitz gave as hosts of his *Sphæria gyrosa Fagus* and *Juglans,* and of *Sphæria radicalis* the exposed roots of *Fagus.* Too much weight, however, should not be placed on the hosts given by Schweinitz, for an examination of fungi of different kinds collected by him shows that in his statements as to the hosts he was not always to be trusted.

The generic position of the chestnut fungus is of interest only to mycologists. It has been placed by Rehm in the genus *Valsonectria* and by Von Hoehnel in *Endothia.* If we accept the distinction between the Hypocreales and the Sphæriales as generally understood, then *Diaporthe parasitica* should be placed in the former and removed from *Diaporthe* proper, which belongs to the Sphæriales. The distinctions between the two groups, it must be confessed, are rather arbitrary in a more natural system, and the valsoid genera of both groups might be put together. But the present condition of mycology does not admit the formulation of a truly natural grouping of genera. If, as I think, *Endothia* should be kept as a genus, then *Diaporthe* should be placed in that genus rather than in *Valsonectria,* which was not created until years later. Furthermore, even if *Diaporthe parasitica* be considered a true *Diaporthe,* the name *Endothia,* it should be remembered, antedates *Diaporthe* of Nitzschke.

In conclusion it may be said that the chestnut-blight fungus suggests a number of important and difficult questions to mycologists. A definite answer to some of them might throw some light on the possibilities of checking the disease, but wherever it may have come from, whether native or exotic, what we now know of its life history unfortunately gives us no reason to suppose that it could be seriously checked, much less extirpated, by any means which could be generally adopted, although something might be attempted where it is desired to protect special limited areas. At present it is the mycologist rather than the forester who is called on to investigate. From what has been said the

following problems remain to be solved. What is the relation of our chestnut-blight fungus to the *Endothia* on chestnuts in Italy? What species related to or identical with the chestnut fungus grow on other trees in this country, and how do they affect such trees? Is it possible to determine authoritatively whether *Sphæria gyrosa* and *Sphæria radicalis* Schweinitz are identical or distinct species, and are European botanists justified in believing that the *Endothia* of Europe is identical with either of the species of Schweinitz? Some of these questions mycologists may be expected to answer hereafter. Others may never be answered except by those in whom the power of observation does not exclude the exercise of a vivid imagination.

W. G. Farlow

MORE TROUBLE FOR THE SYSTEMATIST[1]

On a former occasion, in an address as retiring chairman of Section F of the American Association for the Advancement of Science, your speaker had occasion to bid for the sympathy of his zoological colleagues, the immediate cause of distress being a prediction on the part of Dr. C. B. Davenport that "the future systematic work will look less like a dictionary and more like a table of logarithms."

In the ten years that have passed since that time, this particular specter has not reappeared, and the systematists have placidly gone on their way, apparently oblivious to the existence of logarithmic functions. This, however, may be due to their general belatedness and ultra conservatism; and it is not impossible that the threat of Dr. Davenport may still disturb the placidity of their dreams.

There are other troubles, however, that have arisen in the meanwhile, that are not a whit less disturbing than the one just mentioned.

A serious and most important effort to meet some of the difficulties of nomenclature has been made in the formation of the International Commission on Zoological Nomenclature, a thoroughly dignified and able body of zoologists, of which Dr. C. W. Stiles is the accomplished secretary and most influential American member. In the formation of this commission great pains were taken to make it truly international and representative. It was formally appointed by the most dignified body of zoologists in the world, the International Zoological Congress, and has striven earnestly and faithfully to perform its herculean task. It has been confronted with almost unsurmountable obstacles, and is certainly deserving of praise for its efficiency and courage.

That this commission would meet with serious difficulties was to have been predicted. In the attempt to formulate general laws it is inevitable that there should result individual cases of hardship and injustice, particularly when the law is inflexibly administered. Zoologists, like other men, are apt to be more or less restive under restraint, and consistency in applying the law of priority enacted by the International Commission was bound to involve irritating consequences.

These consequences are felt not only by the relatively small number of systematists, but even more keenly by the morphologists, embryologists and others who have to use zoological names, although they are spared the pains of making them, and are much inclined to cling fondly to those which have been rendered familiar by usage.

These men are naturally exasperated when they are required to call a holothurian a "bohadschioidean," and find it hard to recognize an actinian under the guise of "Dagysidæ."

Systematists have always, however, been subject to the execrations of their fellow zoologists along these lines, and at times deservedly so. It is inevitable, on the one hand, that classifications and hence names must change with the increase of knowledge and, on the other hand, it is equally certain that pedantic systematists and hair-splitting pur-

[1] Read before the Central Section of the American Society of Zoologists, at Urbana, Ill., on April 5, 1912.

ists will arise and, with more zeal than judgment, create havoc with existing and revered classification. These men have no exemption from the common quota of error which afflicts mankind in general, but their mistakes are apt to be more than ordinarily disturbing. There is such a thing as excessive pedantry in every class of students, as there are men who crucify the spirit of the law in order to maintain the letter.

But, to return to the International Committee on Zoological Nomenclature, it must be conceded that it has unraveled skillfully and patiently many knotty problems in nomenclature, and has performed a function which is surely an important one.

But it has aroused a more or less active spirit of opposition by its strict application of the priority law, a rule that is at the very foundation of many of its decisions. This law reads as follows:

Art. 25. The valid name of a genus or species can be only that name under which it was first designated on the condition (*a*) that this name was published and accompanied by an indication, or a definition, or a description; and (*b*) that the author has applied the principles of binary nomenclature.

While there have been individual zoologists who have vigorously objected to the rigid enforcement of the priority rule, it remained for the Scandinavian and Finnish zoologists to make the first formal and organized protest. There was published in the *Annals and Magazine of Natural History* for December, 1911, an article entitled "A Vote against the Strict Application of the Priority Rule in Zoological Nomenclature, with an Introduction by Dr. Th. Mortensen."

This introduction is interesting reading for the insurgents. It reviews the efforts that have been made to induce the International Commission to agree to the recommendation that "certain very commonly used zoological names should be excepted from the law of priority," and states that the Commission on Zoological Nomenclature has shown no inclination to accept the recommendation, claiming that such a desire for exceptions to the rule is not indicated by any great number of zoologists.

It seems that there was published in the number of the *Annals and Magazine of Natural History* for December, 1910, a portion of an advanced copy of the Report of the International Commission in which the commission invites all zoologists to send in, prior to *November 1, 1910,* a list of 100 zoological names. All systematists are invited, moreover, to send a separate list of 50 or 100 generic names in their specialty which they look upon as most important and most generally used, each name to be accompanied by the full and complete bibliographic reference, by the name of the type species and the name of the order and family to which the genus belongs.

This proposition Dr. Mortensen regards as "not very far from an absurdity." Perhaps this language is too strong to apply to a request from the International Commission on Zoological Names; but it is nevertheless exceedingly frank. It would be interesting, moreover, to know how many systematists there are in this body who are so situated that they could drop their ordinary work and supply, on short notice, such a list, with proper bibliographic references.

Dr. Mortensen, with the help of some of his colleagues, secured a vote from 122 professional zoologists in Scandinavia and Finland, and found that all but two of them were ready to sign the following statement:

> The undersigned Scandinavian and Finnish zoologists protest against the strict application of the law of priority in all cases, and express the desire that the most important and generally used names should be protected against any change on nomenclatorial grounds.

The names and official positions of the signers are appended. Dr. Mortensen concludes as follows:

> It is to be hoped that the zoologists of other countries will follow the example given here. When this has been done, and it has been definitely proved that the great majority object to the strict application of the priority rule, it may perhaps be expected that the tyranny of that notorious law, which has already done so much harm to

science, will be thrown off; and then, perhaps, the International Commission will see that it is rather its duty to arrange for the codification of the desired names in accordance with the wishes of the zoologists.

Upon looking up all of the evidence at hand, and also communicating directly with the secretary of the International Commission, I am forced to the conclusion that there is no disposition on the part of that commission to except any names whatever from the mandate of the priority rule.

It should be said here that prior to the protest from the Scandinavian and Finnish zoologists, the British Association and the American Society of Zoologists had recommended "that certain very commonly used zoological names should be excepted from the law of priority," and, aside from these formal actions there has been developed a considerable amount of individual hostility to the strict application of the law; and in some cases there is open revolt.

The practical working systematist is now confronted with a very serious and perplexing dilemma. He is forced to ask himself which of two courses he should pursue. Shall he adopt the ruling of the commission and adhere strictly to the law of priority, in which he will not be followed by a large and important class of his colleagues? Or, shall he use his judgment in each particular case which comes up for decision, and thus bring down upon himself and his work the criticism of another important class and have both condemned by the International Commission on Zoological Nomenclature, a body created by the International Congress, and having plenary powers to enforce this rule? And it must be remembered that a large number of active systematists are thoroughly in accord with the commission.

It surely seems as if the systematist here finds himself placed squarely between the Devil and the deep sea. It is, of course, not within the province of this paper to designate which is which.

Now it must be confessed that there is much to be said in favor of the attitude taken by the commission in this matter. In the first place, there is great virtue in a clear-cut and definite law, one without any "ifs" nor "ands" about it; and this advantage is undoubtedly possessed by the law in question. And it is clearly to the advantage of the commission, as court of last resort, to have such a rule at its back. A law of this kind is administered with much more facility than a looser one, and the consistent administration of such a rule can bring no efficient criticism upon the commission, *provided* that the priority rule has been legally enacted!

As a matter of fact, the International Congress of Zoologists, although perhaps not a strictly legal body, is presumably the most thoroughly representative, indeed the only international body of zoologists in a broad sense, that exists, or has existed.

This body formally adopted the code, as formulated by the duly authorized International Commission on Zoological Nomenclature, at the Berlin meeting, in 1901. The commission itself was formally appointed in 1895 at the Leyden meeting.

Further amendments were submitted by the commission, and adopted by the International Congress at the Boston meeting in 1907. Meanwhile a number of zoologists expressed the wish that the commission serve as a court for the interpretation of the code, and it has consented to act in that capacity.

A careful review of the records and history of the commission has forced upon the writer the conviction that the priority rule has as thorough a sanction in law as can be given by the International Congress of Zoologists, and that there is nothing in the records which authorizes the commission to deviate from that law. Moreover, it appears that the commission is correct in declaring that it "has no legislative power," and it is difficult to see how it could assume the right to practically amend the priority rule.

If ordinary parliamentary usages are to be followed, it seems to be plainly indicated that the only power that can amend or abolish this rule is the International Congress itself, either on its own motion, or in response to a recom-

mendation of the International Commission. But the congress itself has decreed that "no proposition for change in the code is permitted to come before the congress unless it is presented to the Permanent Commission at least one year before the meeting of the congress."

A letter from Dr. Stiles, the secretary of the commission, informs me that the congress has gone on record to the effect that it demands "a unanimous vote of the commission before any matter will be considered by the congress."

It is interesting to speculate at this point whether there is any conceivable method by which a dignified body of scientists could more completely and finally tie its own hands than the one here solemnly consummated by the International Congress of Zoologists. The method, in brief, is as follows:

1. Appoint a commission with power to formulate a code.
2. Formally adopt that code.
3. Forbid any amendment to be introduced except through the commission.
4. Declare that nothing will be considered unless brought before the congress with the unanimous vote of the commission.

I submit, most respectfully, that nothing more perfect of its kind has every been perpetrated by any political machine or autocrat.

All that the commission has to do is to "stand pat." The congress has done the rest.

C. C. NUTTING

HENRY JAMES CLARK: TEACHER AND INVESTIGATOR[1]

HENRY JAMES CLARK, or H. James-Clark as he often wrote his name, sometimes called, not inaptly, the first professor of natural history at this college, was the first trained zoologist to occupy a chair here. But hardly had he entered upon its duties when he was called from this life in the flower of his age.

Born at Easton, Massachusetts, on the twenty-second of June, 1826, the son of a clergyman, his father moved to Brooklyn, N. Y., where he lived many years and where the son received much of his early training and was fitted for college. After completing his preparatory studies, he entered the University of the City of New York, and was graduated thence in 1848. From college he went as a teacher to White Plains, and while engaged in the study of botany, made observations upon the structure of *Chimaphila* and *Mimulus,* which he communicated to Dr. Gray. These and subsequent observations upon the flora of the neighborhood attracted to him the favorable notice of the latter, who invited him to Cambridge. Thither he went in 1850, and enjoyed for a time the advantages of a pupil and private assistant at the botanic garden. While a student there he taught, for a single term, the academy at Westfield, achieving much success as a teacher. Soon after this a taste for zoological studies, developed by the lectures of Professor Agassiz and frequent visits to the zoological laboratory, led him to abandon botany for what appeared the more fascinating study of animal life. Graduating from the Lawrence Scientific School in 1854, he became immediately after the private assistant of Professor Agassiz. Three years later Agassiz spoke of him enthusiastically, remarking to a friend, "Clark has become the most accurate observer in the country." In June, 1860, he was appointed assistant professor of zoology in the Scientific School at Harvard University, a position he held until the expiration of his term of office.

A few weeks following his appointment he went abroad, mainly for his health, traveling in England, France, Germany and Switzerland, often on foot, and visiting the leading universities and museums. He met many scientific workers, including Allman, Alexander Braun, Gegenbaur, Haeckel, Huxley, Leuckart, von Martius, Milne-Edwards, Schleiden, but especially Owen, whose guest

[1] An address delivered at the dedication of the building for entomology and zoology at the Massachusetts Agricultural College, November 11, 1910. In the preparation of this sketch I am indebted to Dr. Edward S. Morse and Professor A. E. Verrill for much valuable information.

he was at Sheen Lodge, Richmond Park. While in Germany he attended the meeting of German Naturalists and Physicians at Königsberg, of which he has left an interesting account in his notes of European travel.

In the spring and summer of 1861 he gave a course of lectures on histology at the Museum of Comparative Zoology. One of his friends writes:

> I remember his interesting lectures before our small class on cellular structure in plants and animals. His skill with the microscope and his rare ability to draw aided him greatly in making out the minutest details of cell structure. His personal qualities were of a kind to endear him to many friends, especially to those students who sought and obtained from him counsel and advice in their studies, as I did on many occasions.

The small class included Hyatt, Morse, Packard, Putnam, Scudder, Shaler and Verrill.

Notwithstanding his constant investigations, Professor Clark found time to prepare a course of twelve lectures—the result of his microphysiological studies—which he delivered at the Lowell Institute in the winter of 1864. These were subsequently rewritten and published in 1865, under the title of "Mind in Nature; or the Origin of Life, and the Mode of Development of Animals." This work, based on structure and development in the animal kingdom, is crowded with original observations and testifies to years of the severest labor and independent thought. "It is in all respects," says Packard in 1873, "for its usually sound and clear thinking, its breadth of view and the amount of original work it contains, perhaps the most remarkable general zoological work as yet produced in this country."

Clark adopted and strongly urged the doctrine of spontaneous generation, from the facts afforded by the experiments of Jeffries Wyman, and on the question of evolution adopted views resembling those of Richard Owen. The original matter in the book is that relating to the structure of *Bacterium termo* and *Vibrio bacillus*, the theory of the egg and its polarity and bilaterality, and the cellular structure of *Actinophrys*, with many other new points relating to the anatomy and physiology of the Protozoa and Radiates. It anticipated also certain points in histology, and the structure of the Protozoa and Sponges especially, which have made the succeeding labors of some European observers notable.

In 1866 Professor Clark accepted the chair of botany, zoology and geology at the Agricultural College of Pennsylvania, where he remained three years, exchanging it in 1869 for similar duties at the University of Kentucky. Neither of these posts was agreeable to his taste, chiefly on account of the pressure of college duties, which left him but little time for abstract investigations. It was, therefore, with great readiness he accepted the call to this college in 1872.

Here his duties were of a more congenial nature, and he applied himself with renewed energy to teaching and soon began the formation of a museum—a working collection of comparative and pathological anatomy. Turning to his first and only report—remarkable for its clearness, particularity and insight—we find that he taught human anatomy and physiology, comparative anatomy and zoology, and comparative physiology. These studies were to form the groundwork for a course in general and veterinary pathology. He lays stress on the importance of the objective method of teaching in the class-room and of laboratory instruction. "Having mastered," he says, "the general principles of structure and relation throughout the length and breadth of the animal kingdom, the rawness of total ignorance is supplanted by a new habit of thought, and a proneness to make further inquiry upon meeting with any object in nature. Here, then, comes the time for laboratory practise. Supplied with scalpel and magnifier, the student should be required to work out topics upon unprepared specimens. If he has acquired the smallest grain of interest in the matter previously, patience will enter where it could not possibly have existed before. He learns the art of seeing and knowing what he looks at; he becomes by degrees an observer; and in doing that, he is

also becoming unconsciously a draughtsman, and when required, as he should be, to produce with pencil what he sees, if he wants to do it, he will do it instinctively." Lastly, Clark had in view graduate courses for advanced work and for special training.

His work was now interrupted by a severe illness. Never robust, his assiduous and confining labors had seriously impaired his health. As early as 1857 are entries in his diary of symptoms indicating that the seeds of the disease that was to cut him off in his prime were then sown. After much suffering, on the first of July, 1873, at the age of forty-seven, his useful life came to an end. He was a member of the leading scientific societies in this country, including the National Academy of Sciences, which up to that time was limited in membership to fifty of the foremost scientists of the country.

His first love for science, writes one who knew him well, seems to have grown from his fondness for flowers. After he became a student of Professor Agassiz his love for botany remained undiminished. He studied it in after years from the side of vegetable histology and morphology in connection with and as illustrating the histology and morphology of animals. The influence of his knowledge of botany on his zoological studies was marked. It prepared him for his studies on spontaneous generation, on the theory of the cell, on the structure of the Protozoa and the nature of protoplasm. In studying the lasso-cells of the acalephs, he traced their analogical resemblance to the stinging hairs of the nettle. By his intimate knowledge of the spores of the smaller algæ he was able to point out some of the characters separating the lowest Protozoa from the spores of plants, and aid in the work of Thuret and others in eliminating from the animal kingdom certain vegetable spores which had been originally described as Infusoria.

In his first scientific paper, communicated by Dr. Gray in 1856, he showed that in most of our North American gentians the ovules are spread over the whole parietes of the ovary, either irregularly or in vertical lines on the veins. His next paper was on the peculiar growth of rings in the trunk of *Rhus toxicodendron,* and this was supplemented by further studies on the eccentricity of the pith of *Ampelopsis quinquefolia* and *Celastrus scandens.* He made experiments for a series of years on the value of the bark to the life of the tree. He observed the relation and development of the filaments which connect the anthers to the sepals of *Comandra umbellata.* In his paper on the identity of the vibrios and the muscular fibrillæ, he showed how the latter during decomposition break up transversely, the fragments assuming the form and movements of the former. He also made observations on the absorption of albumen in the cells of plants. His last purely botanical paper (1859) was on the nature of the glandular dots of the pine. His skill in the use of the fine lenses made by Spencer (under his direction) enabled him to see more than his predecessors of the true relations of these dots. But his botanical studies did not end here, as may be seen by reference to his diaries and his frequent allusions to the lower algæ and to vegetable histology in "Mind in Nature." In his walks he often botanized, and contributed in this way to Gray's botanical text-books. Thus with the training he received from Gray and Agassiz, he looked upon the world of organized beings from both the botanical and zoological side. He well deserves the name, *biologist.*

Between 1856 and 1863 he was associated with Agassiz in the preparation of the anatomical and embryological portions of the great work entitled "Contributions to the Natural History of the United States." To these volumes he was a large contributor, most of the histological and embryological portions of the work being his, and more than half the plates illustrating the embryology and histology of the turtles and acalephs bear his name. "In the preparation of this part of my work," says Professor Agassiz, "I have received much valuable assistance from my friend and colleague Professor H. J. Clark, who has traced with me, for more than nine years, the metamorphoses of our Acalephs,

and especially those of the Hydroids. He discovered the peculiar structure of the lasso-cells of the Ctenophoræ."

During this time Clark began the serious study of the Protozoa, undoubtedly compelled to do so in order to properly interpret the histological facts then accumulating in the study of the Radiates. After leaving Cambridge he studied the Infusoria and lower plants, and made drawings and notes comprising descriptions of many new forms of Infusoria. He planned an extensive work upon this subject, which, had he lived to complete it, might have equalled if not surpassed Claparède and Lachmann's famous work on the Infusoria. He did not dissociate the Protophyta from the Protozoa, regarding them as almost inseparable in nature; thus, in his lectures to his classes, well nigh anticipating Haeckel's classification of the lowest forms of the animal and vegetable kingdom into the Protista and Protozoa.

In his first paper on *Actinophrys* (1863) he announced the discovery that "all vibratile cilia originate in the amorphous intercellular substance," and do not form direct prolongations of cells, *i. e.*, that cilia are prolongations or extensions of the protoplasmic substance of the cells from which they arise. The same year he discovered the eggs of *Tubularia,* and showed that there was but one type of development in the Hydromedusæ (excepting the Narcomedusæ and Trachymedusæ), and that the differences observed in the developmental process were merely modifications of degree and not of kind, an exceedingly valuable addition to our knowledge of the affinities of the various groups of Hydromedusæ.

Foremost, perhaps, among his several discoveries with the microscope was that of the true nature of the cilio-flagellate infusorians and the sponges. In 1866 appeared a brief paper, entitled "Conclusive Proofs of the Animality of the Ciliate Sponges, and of their Affinities with the Infusoria Flagellata." While Clark had endeavored to show in his Lowell Lectures that there was a unity of plan in the organization of the Protozoa, their bodies being arranged in the form of a helix, he now endeavored to prove that the sponge did not depart from the protozoan type. In the full memoir, published about a year later, under the title "Spongiæ Ciliatæ as Infusoria Flagellata," he attempted to establish the homology of the flagellate cells of the sponge with the flagellate Infusoria. His discovery of the flagellated cells of living sponges and demonstration of their animal nature was a great step in advance of previous observers. While, as Clark observes, Carter had first detected the true criterion of their animality, this was confirmed and demonstrated still more completely by Clark himself, as acknowledged by Carter in his "Confirmation of Professor James-Clark's Discovery of the True Form of the Sponge-cell (Animal)."

The Choanoflagellata, or collar-bearing flagellate animalcules, were discovered by Clark, and his further discovery that the flagellated (ciliated) chambers of sponges are lined by collared cells of the same peculiar structure as the individual Choanoflagellata, led him to regard the sponges as colonies of Choanoflagellata. The views maintained by Clark with reference to the position and affinities of the sponges were, that these organisms must be regarded as compound colonial forms of Flagellata, whose units, in the case of *Leucosolenia,* exhibited a type of structure essentially similar to that of *Codosiga* and *Salpingœca*—genera established by him to receive his collared cell forms—but might possibly in other instances more closely approximate to that of *Monas* (*Spumella*) *Bicosœca* or *Anthophysa.* In these views he was supported by the observations of Saville Kent and Stein, and in the main by those of Carter and to a less extent by Balfour, but opposed by Haeckel and F. E. Schulze. The subsequent discovery by Saville Kent of *Proterospongia* (*Savillia*) at that time rendered the derivation of the sponges from the Flagellata at least a tenable hypothesis, while Balfour considered them as an intermediate group between the Protozoa and Metazoa.

In the last paper he published Clark compared the arguments adduced by Haeckel in

favor of the cœlenterate affinities of the sponges with the actual structural composition of *Spongilla* and *Leucosolenia,* and reached the conclusion that the relationship of the sponges to certain flagellate Protozoa was so distinct and decisive as to forbid their logical inclusion among the representatives of any other class. The universal and characteristic collared cells of sponges point emphatically to a choanoflagellate ancestry and, as a recent authority has observed, in the present state of our knowledge it would be difficult to frame a definition of the Protozoa which should absolutely exclude the sponges. His work on flagellate Protozoa and sponges was a valuable contribution to science. Since his time great advance has been made in our knowledge of the histological structure, mode of reproduction and embryological development of these organisms, due mainly to the opportunities offered by novel and refined methods of technique. Had the art of making thin sections and the staining of tissues been known in his day, he would certainly have anticipated much of the later work of cytologists and embryologists.

In the use of the microscope, Clark showed not only mechanical skill and ingenuity, but a patience, caution, and experience in difficult points in histology, which undoubtedly placed him at the head of observers in this country and rendered him perhaps inferior to few in Europe. He used the highest powers with a skill that few if any living observers have surpassed. He suggested improvements carried out by Spencer and Tolles in this instrument. In 1857 Professor Agassiz sent him to Canastota to confer with Spencer, and as a result a microscope was made by him which was fully equal to any made at that time in Europe. Clark suggested that we must have three kinds of objectives: one with the field extremely flat; another, an immersion lens—the first made, so far as we are aware, and now so universally used; and a "third with a depthing focus extending as far as possible beyond that of the ordinary kind, for the purpose of viewing objects as a whole, in order to ascertain the relations of their different parts." This microscope was in use in 1859.

In 1878, five years after the death of Professor Clark, the Smithsonian Institution published, as one of its Contributions to Knowledge, his monograph of the "Lucernariæ and their Allies." This group was the subject of his last studies, though one which had early engaged his attention. Complete and elaborate as it is so far as it extends, this beautiful memoir is only a fragment of what was evidently designed to cover at least fifteen parts, two parts only having an actual existence. It has been well said that a broken shaft would represent both the author's life and this posthumous work, each symmetrical and thoroughly finished to the point where they suddenly broke off.

The Lucernarians were not regarded by Clark as truly radiate animals, but in a degree bilateral, with a fore and hinder end. The commonly received theory at that time that the so-called Radiata are founded upon the idea of radiation, was combated by him in 1865. The views of the present day coincide in the main with his—that radiation is, on the whole, a superficial feature, not always constant in Cuvier's Radiata, though often well marked. He regarded the so-called polymorphic individuals as "organs under various disguises," and he ascribed a high degree of individuality to the jelly-fish, *Pelagia,* and only a less amount to *Lucernaria.* He believed with the advanced histologists of his day that "*cells* so-called (no matter whether constituted according to the older histologists or according to the most recent theory) are, after all, of secondary importance, and that the *cytoblastema* or protoplasm (which we do not distinguish from inter-cellular substance) is in the main an *essential element,* the potential progenitor of all tissues, and that it projects itself into the utmost feature of the living body by a process of self-proliferation. Through this, and this only, can a true *law of continuous development* be illustrated; while the various forms of cell-tissue, and fiber-tissue, and bone-tissue, etc., are but the disjointed collateral developments,

each one irrespective of the other, from the continuous, onward stream of cytoblastema." He says, further, that "all Rhizopods are moving, sentient masses of *Cytoblastema*, and that alone."

Clark was admirably adapted by nature for doing histological work of the highest order. He possessed that philosophic insight of the true naturalist which often enables him to *divine* much further than he can perceive in the tracing of relationships and to anticipate what the microscope is to reveal. At the time he began his work as an observer, zoological science in America was especially deficient in histological investigations, and he did more than any one else to remove this reproach upon American biology.

"Henry James Clark," said Asa Gray at the time of his death, "deserves to be enrolled in the list of botanists. Although his high reputation was won in another department, he was an excellent botanist before he became Mr. Agassiz's assistant and gave himself to zoological investigation: the present writer was indebted to him for more than one interesting discovery of points of structure. He is thought to have been the ablest microscopic investigator which this country has produced." "His labors as a zoologist," said James D. Dana, "especially in those departments requiring difficult microscopic research, had placed him among the two or three first in the country, and given him a world-wide reputation. He was always working, and full of enthusiasm in science, and also a most genial and excellent man."

In the preface to volume one of the "Contributions to the Natural History of the United States," dated October 3, 1857, Professor Agassiz says:

> Mr. H. James Clark has assisted me from the beginning of my investigation of the embryology of these animals, and drawn, with untiring patience and unsurpassed accuracy, most of the microscopic illustrations which adorn my work. I owe it to Mr. Clark to say, that he has identified himself so thoroughly with my studies since he took his degree in the Lawrence Scientific School, that it would be difficult for me to say when I ceased to guide him in his work. But this I know very well, that he is now a most trustworthy observer, fully capable of tracing for himself the minutest microscopic investigation, and the accuracy of his illustrations challenges comparison.

In a lecture given at the summer school of Natural History at the Island of Penikese, early in July, 1873, Professor Agassiz announced the death of Professor Clark and spoke in the highest terms of his work and of him as a man. He said that as a microscopist and histologist he regarded him as fully equal if not superior to Ehrenberg, who was at that time considered the best in the world.

Professor Fernald, to whom we owe the department of entomology in this college and in whose honor we are gathered to-day, has expressed the opinion that he was the most brilliant of all the young men that Agassiz drew round him when he first came to this country; that as an artist in illustrating his work he probably had few if any equals, and as an observer and investigator he must be placed in the front rank of the naturalists of his time. In marine zoology he probably had no superior, and if his life had been spared he would undoubtedly have left a marked impress on the college.

Three things can be said of Clark. He was a genius, he had the best of teachers, and he made the most of his opportunities. The secret of his success as an investigator may be stated in his own words taken from his diary, where he says, "I made it a rule to practise the utmost rigidity and thoroughness in my researches, without regard to time consumed or the value of the results." Such was the life of Henry James Clark, full of supreme devotion to science and showing an indomitable energy in the search for truth.[2]

Frederick Tuckerman

[2] Several species bear his name. Agassiz dedicated to him the acaleph, *Idyiopsis clarkii;* Bütschli the animalcule, *Salpingœca clarkii*—a name later bestowed by Stein upon another form of the same genus. His services are also commemorated in *Ascortis clarkii* of Verrill, the most delicate species of calcareous sponge found on our coast.

SCIENTIFIC NOTES AND NEWS

COMMEMORATION day will be observed by the University of Glasgow on June 25, when Professor F. O. Bower, F.R.S., will deliver an oration on "Sir Joseph Hooker."

THE Aero Club of Washington held a field day last Monday in commemoration of the anniversary of Secretary Langley's first aerodrome flight on May 6, 1896. Professor Willis L. Moore, president of the club, gave an address on Langley's work and influence on aviation. General Robert Shaw Oliver spoke on the work of the army in aviation, and Captain W. I. Chambers, on the work of the navy.

A PORTRAIT of Dr. Edgar F. Smith, provost of the University of Pennsylvania, painted by Mr. Hugh H. Breckenridge, will be presented to the university by the members of the class of 1902 College on the occasion of their tenth anniversary this June.

THE speaker at the annual commencement exercises of the Johns Hopkins University on June 11 will be Dr. William C. Gorgas, chief sanitary officer of the Panama Canal Zone and member of the Isthmian Canal Commission.

DR. WILHELM FIEDLER, professor of mathematics at Zurich, has celebrated his eightieth birthday.

MAJOR VON ABERCRON, known for his work in aeronautics, has been made an honorary doctor by the University of Marburg.

DR. ADOLF VON KOENEN, professor of geology at Göttingen, has received the honorary doctorate of engineering from the Technological Institute of Hanover.

AT the celebration of the seventy-fifth anniversary of the foundation of the University of Athens, on April 10, honorary degrees in medicine were conferred, as reported in *Nature*, on Professors von Behring (Marburg), Celli (Rome), Ehrlich (Frankfort), Exner (Vienna), Golgi (Pavia), Kronecker (Berne), Laudouzy (Paris), Richet (Paris), Sir Ronald Ross (Liverpool), Roux (Paris), Schulze (Würzburg), Weichselbaum (Vienna) and others. The degree of doctor of philosophy was conferred on Sir Donald MacAlister (Glasgow), Delbrück (Jena), Dörpfeld (Athens), Gubernatis (Rome), Harnack (Berlin), Kenyon (London), Mahaffy (Dublin), Wheeler (Berkeley) and others; and the degree of doctor of science on Professor Depéret (Lyons), Halácsy (Vienna), Lacroix (Paris), Lepsius (Darmstadt), Partsch (Leipzig) and Philippson (Bonn).

THE third annual award of the Hunterian Society's medal has been made to Dr. A. Goulston, of Heavitree, Exeter, for his essay on "The Use of Sugar in Heart Disorders."

GRANTS have been made from the C. M. Warren Fund of the American Academy of Arts and Sciences as follows: To Professor H. G. Byers, University of Washington, $250, for work upon the passivity of metals. To Professor W. D. Harkins, University of Montana (to be at Chicago University), $300, for work upon the energy relations in a surface between two liquid phases. To Dr. Latham Clark, Harvard University, $150, for work on the paraffin hydrocarbons.

MR. R. N. LYNE, director of agriculture in Portuguese East Africa, has been appointed the director of the new agricultural department of Ceylon.

DELEGATES have been appointed by the Academy of Natural Sciences of Philadelphia as follows: To the Second International Congress of Entomology, Dr. Henry Skinner, Professor Philip P. Calvert and Dr. W. J. Holland; to the Eighteenth International Congress of Americanists, Sir Thomas Lauder Brunton.

DR. EDWARD BARTOW, director of the State Water Survey and professor of chemistry at the University of Illinois, has been appointed a delegate to the fifteenth International Congress of Hygiene and Demography, to be held in Washington, September 23 to 28.

DR. H. B. WARD, head of the zoological department of the University of Illinois, has been appointed delegate of the Illinois Academy of Science to the meetings of the Iowa Academy of Science at Des Moines. He will deliver two addresses.

The New York Section of the American Chemical Society has appointed a committee on occupational diseases in chemical trades as follows: Charles Baskerville, professor of chemistry, College City New York, *Chairman;* E. C. Uhlig, chief chemist, Brooklyn Union Gas Co., Brooklyn, *Secretary;* George P. Adamson, Baker and Adamson Chemical Co., Easton, Pa.; W. H. Bassett, American Brass Co., Waterbury, Conn.; Wm. F. Doerflinger, consulting chemist, 52 Beaver St., New York City; H. M. Kaufman, Mutual Chemical Company of America, 55 John St., N. Y.; A. C. Langmuir, chairman of the New York Section, American Chemical Society, 9 Van Brunt St., Brooklyn, N. Y.; Geo. A. Prochazka, general manager Central Dye Stuff and Chemical Company; Geo. D. Rosengarten, Powers, Weightman and Rosengarten, Philadelphia, Pa.; A. H. Sabin, consulting chemist for the National Lead Company, 129 York St., Brooklyn, N. Y.; Charles L. Parsons, mineral chemist, Bureau of Mines, Washington, D. C. This committee will work in cooperation with the committee of the Association for Labor Legislation.

Dean Eugene Davenport, of the College of Agriculture of the University of Illinois, was the speaker at the recent commencement exercises of the University of Nebraska College of Agriculture.

R. G. Dukes, professor of applied mechanics at Purdue University and second of the exchange lecturers with the University of Illinois, lectured April 26 on "The Recent Discoveries in Physical Science and their Bearing on the Progress of Engineering." Mr. O. P. Hood, who is mechanical expert for the United States Bureau of Mines at Pittsburgh, also addressed the meeting.

Dean C. H. Johnston, of the University of Kansas, gave three lectures before the School of Education of the University of Illinois on April 29 and 30 on present problems in high school organization.

The annual commencement address of the Missouri School of Mines will be given by Mr. James R. Finlay, of New York City. Exercises will be held on the morning of May 31. The graduating class numbers forty.

The University of Pennsylvania Chapter of the Society of the Sigma Xi initiated twenty-nine new members on April 23. Among them were Professor Samuel Christian Schmucker, of the West Chester State Normal School, and Mr. Witmer Stone, a curator in the Academy of Natural Science in Philadelphia. Professor George Hervey Hallett, of the University of Pennsylvania, addressed the Chapter on the subject of "Hyperspace." Professor George F. Sever, president of Columbia chapter, was present as a delegate from that chapter.

The Museum of the University of Pennsylvania has arranged to send an expedition to the Amazon under the direction of Mr. Algot Lange. The expedition will be furnished with a steamer which will be large and seaworthy enough to carry the party from New York to the mouth of the Amazon and up the Amazon for several thousand miles to the tributaries where the Indian tribes will be studied and collections made for the next three years.

Mr. George Borup, a graduate student in geology at Yale University, and Mr. S. W. Case, a graduate student in mining, were drowned in Long Island Sound on April 28, by the overturning of a power canoe. Mr. Borup accompanied Commander Peary in his Arctic expedition, and had planned to undertake this summer with Mr. D. B. McMillan, an expedition to Crocker Land, as described in a recent issue of Science.

The heirs of the late Frau Adelheid Bleichröder have given the German Association of Physicians and Scientific Men $25,000 for the support of research work in medicine and the sciences contributory to medicine.

The provisional arrangements for the forthcoming celebration of the 250th anniversary of the Royal Society, as given in *Nature,* are as follows: Monday, July 15—An evening reception of delegates at the rooms of the Royal Society. Tuesday, July 16—In the

morning a commemorative service in Westminster Abbey; in the afternoon the official reception of delegates at the Royal Society and presentation of addresses; in the evening a commemorative dinner at the Guildhall. Wednesday, July 17—In the morning visits to places of interest in London; in the afternoon the Duke of Northumberland gives a garden-party at Sion House; in the evening a conversazione in the rooms of the Royal Society. Thursday, July 18—In the morning visits to places of interest in London; in the afternoon H.M. the King gives a garden-party at Windsor, to which the delegates and fellows of the society will be invited. Friday, July 19—The delegates will visit Oxford and Cambridge Universities.

THE coming of age of the Babcock butterfat test, which was invented by Professor S. M. Babcock, of the University of Wisconsin, twenty-one years ago, has been recognized by the university in a new bulletin written by Dean H. L. Russell, of the College of Agriculture, in which he gives the history and growth of the use of the test throughout the world. Pictures of the original testing machine, now in the dairy school of the university, and of the latest improved forms of testers are shown in the bulletin.

THE prize of the foundation George Montefiore will be awarded 1914 for the scientific advancement or application of electricity. The works may be printed or in manuscript and must be in French or English. The value of the prize is $4,000 and the works must be received not later than March 31, 1914.

THE Warren Triennial Prize, founded by the late Dr. J. Mason Warren, of Boston, in memory of his father, will be awarded in 1913 for the best dissertation on some subject in physiology, surgery or pathological anatomy, the arbitrators being the physicians and surgeons of the Massachusetts General Hospital. The amount of the prize is $500.

THE surgeon general of the army announces that preliminary examination for the appointment of first lieutenants in the army medical corps will be held on July 15, 1912, and September 3, 1912, at points to be hereafter designated. Full information concerning these examinations can be procured upon application to the "Surgeon General, U. S. Army, Washington, D. C." The essential requirements to securing an invitation are that the applicant shall be a citizen of the United States, shall be between twenty-two and thirty years of age, a graduate of a medical school legally authorized to confer the degree of doctor of medicine, shall be of good moral character and habits, and shall have had at least one year's hospital training, after graduation. The examinations will be held concurrently throughout the country at points where boards can be convened. Due consideration will be given to localities from which applications are received, in order to lessen the traveling expenses of applicants as much as possible. The examination in subjects of general education (mathematics, geography, history, general literature and Latin) may be omitted in the case of applicants holding diplomas from reputable literary or scientific colleges, normal schools or high schools, or graduates of medical schools which require an entrance examination satisfactory to the faculty of the Army Medical School. In order to perfect all necessary arrangements for the examination, applications must be complete and in possession of the adjutant general at least three weeks before the date of examination. Early attention is therefore enjoined upon all intending applicants. There are at present sixty-eight vacancies in the medical corps of the army.

THE trustees of Phillips Academy, Andover, Mass., have authorized the department of archeology to begin an archeological survey of Maine this year. Already the curator, Mr. W. K. Moorehead, has had an agent of the department in Maine for some weeks. A number of camp sites, cemeteries and other places have been entered on the standard government maps. It is proposed to map the entire state. The indications are that the ancient Indian population was most numerous along the coast, about Sebago Lake, on the lower Penobscot, Moosehead Lake and Cham-

berlain Lake. A number of students will accompany Mr. Moorehead and others to Maine in June. The excavation of one or two sites will be begun in May.

The University of Michigan Museum of Natural History will send an expedition to Nevada in July and August to secure zoological material for research and illustrative purposes. The investigations will be carried on as the museum conducts all of its field work outside of the state, that is, only a few groups and a small area will be considered and these will receive detailed study. One half of the expense of the expedition will be provided by Mr. Bryant Walker and one half by the university. The museum will also this summer begin a biological survey in the northern part of Chippewa County, Michigan. Hon. George Shiras 3d has provided for the expenses that will be incurred in the preliminary work this summer, and the Michigan Geological and Biological Survey and the museum will complete the study.

The program of business for the eighteenth annual meeting of the British Medical Association in Liverpool has been issued. The representative meeting will begin on July 19. The annual general meeting will take place on the afternoon of July 23, and the president, Sir James Barr, will deliver his address in the evening. The sectional meetings will be held on July 24, 25 and 26. The address in medicine will be delivered by Dr. G. A. Gibson, of Edinburgh, and the address in surgery by Mr. F. T. Paul, of Liverpool. The scientific program of the meeting will be conducted in 20 sections.

The Anglo-American Medical Association, which was founded in Berlin some eight or nine years ago and reconstituted in 1910, has recently acquired, as we learn from the *British Medical Journal,* well-situated quarters of its own at the Hotel Atlas, 105, Friedrichstrasse. They are open daily from 12 to 2 for luncheon; the assistant secretary is in attendance from 1:30 to 4:30 to give information; and from 9 P.M. to 12 P.M. the rooms are open for social purposes, writing and perusal of the journals with which the association keeps itself supplied. In addition, there is a formal meeting each Saturday at 8 P.M., when dinner is taken and a paper read and discussed. One of the special objects of the association is to make the visits of British and American medical men to Berlin pleasant and informing by putting them promptly in touch with what is going on in medical and surgical circles, and by enabling them to meet on a social footing their colleagues in Berlin. It publishes a year-book, the current issue of which contains, in addition to other notes, information as to courses on various subjects more or less constantly in progress, and also as to vacation and other special courses. The honorary secretary and treasurer of the association is Mr. H. R. Carstens.

The Smithsonian Institution reports the completion of the fish collecting in the Panama Canal Zone, and the safe return of one of its representatives. Several of the specialists who have been making collections in this region have been back some time, but the members of the party who have been collecting fishes did not complete the work until early in April, remaining three months in the field. This branch of the work has been supported by the cooperation of the U. S. Bureau of Fisheries and the Field Museum of Natural History, Chicago, Mr. S. F. Hildebrand representing the former and Dr. S. E. Meek the latter. After the work was finished, and the collections shipped to Washington, Dr. Meek went on a visit to Costa Rica, while Mr. Hildebrand returned to Washington, where he arrived on April 13. Mr. Hildebrand reports a most successful trip, and feels confident that all the important fishes of the Canal Zone are represented in the collections which fill some 5 or 6 barrels. The weather was favorable and the work was greatly facilitated by the many privileges and courtesies extended by the Canal Commission. Quarters were furnished by the commission, and each member of the survey was supplied with the regular hotel and commissary books, according him the privileges of an employee.

Living under these arrangements, the party was enabled to secure excellent meals at a very low cost. Besides the work in the Canal Zone proper, the members of the survey made trips into the neighboring territory in search of specimens and data not available within the actual bounds of the zone, but obtainable in the same faunal area or an adjoining one. One of these expeditions carried the party into the Darien country and along the banks of the Rio Tuyra, which lies on the Pacific side of the isthmus about 150 miles distant from Panama. The fauna of this country was found to differ considerably from that of the Canal Zone. The fish collectors, unlike those concerned with some other classes of animals, find it impracticable to classify their specimens while in the field, and for this reason it is difficult to say just what has been obtained, other than that a good-sized collection was made, fully as large as that of last year. The fish were caught in nets and traps of several kinds; often in great numbers, the collectors selecting the best and most desired specimens which are at once prepared and packed in tanks for shipment to the National Museum. Among the different kinds of salt-water fishes secured were sharks, tarpon, jew-fish, snappers, groupers and croakers. There were also many of the cat-fishes which are so very abundant on the Pacific slope and along the coast of Panama. The collectors point out the special value of this survey in so far as concerns the fish fauna which will become completely changed and intermixed when the canal is finished, and the waters of the inland lakes and the two oceans become intermingled. As is well known, many salt-water fishes ascend fresh-water streams for long distances, and it is now seen that in this manner many will doubtless find their way to the great Gatun Lake. Some will probably remain here, while others will pass on into the ocean. At present the fish faunas of the Atlantic and Pacific slopes and the two coasts of the zone appear well defined, but it is believed that a subsequent survey, in from five to ten years time, will reveal great changes.

UNIVERSITY AND EDUCATIONAL NEWS

THE late Dr. Francis Bacon, a physician of New Haven and connected with the Yale Medical School, has bequeathed $100,000 to the New Haven County Antituberculosis Society. He has made Yale University his residuary legatee, the bequest to be used as a scholarship fund in the college.

MR. A. W. OPPENHYM, of New York, by his will created a trust fund of $275,000, which at his widow's death shall go in equal parts to the Mt. Sinai Hospital, the German Hospital and Columbia University. It is provided that the Columbia University fund is to be used for the research into the cause, prevention and cure of cancer.

MR. JOHN TATLOCK, of New York, has given to Columbia University a collection of astronomical works consisting of 261 volumes and about 1,200 pamphlets.

AGRICULTURAL HALL, just built by the University of California at a cost of $200,000 to house some portion of its teaching and investigating in agriculture, will be completed by commencement, May 15, 1912. This new white granite building, roofed in red mission tile, and picturesquely set on an eminence looking south toward the Dana Street entrance to the campus, is one of the permanent buildings of the Phoebe A. Hearst plan. It is fireproof in construction, with steel frame and concrete floors and roof.

THE bill appropriating two hundred and fifty thousand dollars for the development of work in public health and medicine at the site of the University of Illinois in Urbana-Champaign, failed of passage in the special session of the legislature. Although only four votes were recorded against it; yet owing to the fact that at a special session a two-thirds majority is necessary to pass an appropriation bill, involving in this case thirty-four votes, and at no time were more than thirty-five members present, it made it possible for even two votes to defeat the bill. The trustees will renew their request for an adequate appropriation for scientific work in medicine at the next session of the legislature.

Professor Gordon H. True has been appointed director of the Nevada Experiment Station, at Reno.

Dr. Arthur B. Lamb, professor of chemistry and director of the Havemeyer Chemical Laboratory of New York University, has been appointed assistant professor of chemistry at Harvard University. Professor Solon I. Bailey has been promoted to the Philips chair of astronomy vacant by the retirement of Professor Arthur Searle, and Dr. Charles Palache has been promoted to a full professorship of mineralogy.

Dr. Ira W. Howerth, of the University of Chicago, has been appointed professor of education and director of university extension in the University of California. Dr. J. C. Merriam has been promoted to a full professorship of paleontology.

Dr. W. M. Conger Morgan, assistant professor of chemistry at the University of California, has been appointed professor of chemistry in Reed College.

Dr. Franz Doflein, associate professor of zoology at Munich, has been called to the chair of zoology at Freiburg.

DISCUSSION AND CORRESPONDENCE

NON-EUCLIDEAN GEOMETRY IN THE ENCYCLOPÆDIA BRITANNICA

The sixth heading under the word Geometry is Non-Euclidean Geometry. The article is by Whitehead and Bertrand Russell, the best men in England to have written it, and is worthy this one of the three greatest works of reference in the English tongue, the others being Murray's dictionary and the Century.

It begins:

> A short historical sketch will . . . describe the famous and interesting progress of thought on the subject.

But first it gives characteristic properties, beginning with Bolyai's space.

> The sum of the three angles of a triangle is always less than two right angles. The area of the triangle ABC is $\lambda^2(\pi-A-B-C)$. If the base BC of a triangle is kept fixed and the vertex A moves in the fixed plane ABC, so that the area ABC is constant, then the locus of A is a line of equal distance from BC. This locus is not a straight line.

I have called it an *equidistantial.*

> The angle A [which a perpendicular to one of two parallels makes with the other] is called by N. I. Lobatchewsky the "angle of parallelism."

Here as everywhere else in the spelling of Lobachevski's name, the authors have made a very regrettable slip. Lobachevski transliterated his own name into French as Lobatcheffsky, and so it stands in the "édition de Kasan," 1886.

In 1869 Potocki transliterates the name into French as Lobatchefsky, and this spelling is used in the French prospectus issued at Kazan to found the great Lobachevski prize; and the volume "In Memoriam N. I. Lobatschevskii," bears as subtitle, Collection des mémoires présentés à la Société Physico-mathématique de Kasan pour la fête de l'inauguration du monument de Lobatchefsky (1/13 Septembre, 1896) par Mm. Hermite, Halsted, Girardville, Laisant, Lemoine, Neuberg, Ocagne.

My contribution I wrote while sojourning in Kazan, where I had abundant opportunities to learn the name. Gino Loria adopts in Italian the spelling Lobatscheffsky. Now Lobachevski himself also transliterated his name into German, and it stands on the title page of the original edition of his Geometrische Untersuchungen as Lobatschewsky. But Stäckel and Engel Germanize it as Lobatschefskij, the abomination ij being an attempt to represent the i, as in Italian, and the ĭ, very short, with which the name ends in Russian. My friend Sommerville falls into this pit, and spells the name Lobačevskij. Had he dropped that j and replaced his fifth letter by its exact equivalent, our ch as in church, he would have had the proper English transliteration, Lobachevski.

If we be willing to permit in the Encyclopædia the final y, still as English its t is superfluous and its w is indefensible, so that, as the name occurs 25 times, there are 50 places where the quicker the stereotype plates

are corrected the better. Let not the name of a world hero be bungled in the world language, English.

The theory of parallels as it exists in hyperbolic space has no application in elliptic geometry. But another property of Euclidean parallel lines holds in elliptic geometry, and by the use of it parallel lines are defined. Thus throughout every point of space two lines can be drawn which are lines of equal distance from a given line l.

This property was discovered by W. K. Clifford. The two lines are called Clifford's right and left parallels to l through the point.

In both elliptic and hyperbolic geometry the spherical geometry is the same as the "spherical trigonometry" in Euclidean geometry.[1]

The historical sketch is blemished by the unwarranted prominence it gives to Gauss. It says:

We find him in 1804 still hoping to prove the postulate of parallels. In 1830 he announces his conviction that geometry is not an a priori science; in the following year he explains that non-Euclidean geometry is free from contradictions, and that, in this system, the angles of a triangle diminish without limit when all the sides are increased. He also gives for the circumference of a circle of radius r the formula $\pi k(e^{r/k} - e^{-r/k})$.

[In this formula the Encyclopædia has a misprint.]

But all that and immensely more had been given by John Bolyai in 1823 and by Lobachevski in 1826, and published in 1829, while as our authors themselves say, "Gauss published nothing on the theory of parallels."

Then comes the most offensive clause:

It is not known with certainty whether he influenced Lobachevski and Bolyai, but the evidence we possess is against such a view.

But it *is* known that he did *not*, and the evidence we possess against any such influencing is absolute and final. The very next sentence is the opening one of my Translator's Preface, 1891:

Lobachevski was the first man ever to publish a non-Euclidean geometry.

Of Bolyai's work is said:

[1] See chapter XVI., Pure Spherics, in my "Rational Geometry."

Its conception dates from 1823. It reveals a profounder appreciation of the importance of the new ideas, but otherwise differs little from Lobachevski's. Both men point out that Euclidean geometry is a limiting case of their own more general system.

[The Encyclopædia, by a misprint, has *as* for *is*.]

The works of Lobachevski and Bolyai, though known and valued by Gauss, remained obscure and ineffective until, in 1866, they were translated into French by J. Hoüel.

Bolyai was not translated until 1868. Not only were these known to Gauss, but I called attention to the very significant fact that the striking work of Saccheri, truly a non-euclidean geometry, was in the Göttingen library and freely accessible to Gauss during the years 1790–1800. See Gino Loria,[2] who says of Gauss:

Ignoto fino a qual punto egli siasi spinto nella nuova via, come è ignoto se egli abbia ricevuto qualche ispirazione dall' opera del Saccheri che esisteva a Gottinga negli anni 1790–1800 (essendo segnata con un asterisco nella *Bibliotheca mathematica* del Murhard).[3]

If figures are to be freely movable, it is necessary and sufficient that the measure of curvature should be the same for all points and all directions at each point. Where this is the case, it a be the measure of curvature. . . .

This *it* should be *if*.

If a be positive, space is finite, though still unbounded, and every straight line is closed—a possibility first recognized by Riemann.

This, as it stands, is a mistake. On page 24 of von Staudt's "Geometrie der Lage" (1847) we read:

Eine Gerade erscheint hiernach . . . als eine geschlossene Linie.

The possibility first recognized by Riemann is that straight lines may be finite.

On page 729 occurs the long dead phrase "anharmonic ratio," now happily superseded everywhere by Clifford's "cross ratio."

[2] Il passato ed il presente delle principali teorie geometriche. Terza edizione, 1907, pp. 286–287.

[3] Osservazione fatta dall' Halsted nell' articolo "The Non-Euclidean Geometry Inevitable" inserto in *The Monist*, July, 1894.

It is explained in section VII. in what sense the metrical geometry of the material world can be considered to be determinate and not a matter of arbitrary choice. The scientific question as to the best available evidence concerning the nature of this geometry is one beset with difficulties of a peculiar kind. We are obstructed by the fact that all existing physical science assumes the Euclidean hypothesis. This hypothesis has been involved in all actual measurements of large distances, and in all the laws of astronomy and physics. The principle of simplicity would therefore lead us in general, where an observation conflicted with one or more of those laws, to ascribe this anomaly, not to the falsity of Euclidean geometry, but to the falsity of the laws in question. This applies especially to astronomy. . . . But astronomical distances and triangles can only be measured by means of the received laws of astronomy and optics, all of which have been established by assuming the truth of the Euclidean hypothesis. It therefore remains possible that a large but finite space constant, with different laws of astronomy and optics, would have equally explained the phenomena. We can not, therefore, accept the measurements of stellar parallaxes, etc., as conclusive evidence that the space constant is large as compared with stellar distances.

Finally, it is of interest to note that, though it is theoretically possible to prove, by scientific methods, that our geometry is non-Euclidean, it is wholly impossible to prove by such methods that it is accurately Euclidean. For the unavoidable errors of observation must always leave a slight margin in our measurements. A triangle might be found whose angles were certainly greater, or certainly less, than two right angles; but to prove them *exactly* equal to two right angles must always be beyond our powers.

This I have been publishing for the past 35 years in articles some 77 of which, not counting translations, Sommerville has registered in his Bibliography of non-euclidean geometry, 1911. But just here a former pupil of mine, Dr. R. L. Moore, has gone beyond his teacher. His results seem to be unknown to the Encyclopædia, though I called attention to them in Science, October 25, 1907, under the "scare" heading, "Even Perfect Measuring Impotent."

In the brief bibliography appended to this section VI., I notice a number of errors. In the title of Engel's book the *y* should be *ij*. In the title of Dehn's article, the word Legendarischen should be Legendre'schen. In the title of Barbarin's book the capital G and capital E should be lower case letters, and the hyphen should be omitted.

In the title of Bonola's book the capital E should be lower case.

In the title of the article by E. Study the nicht-Euklidische should be Nicht-Euklidische. This title upon a pamphlet of 97 pages [Greifswald, 1900] is Über Nicht-Euklidische und Linien-Geometrie.

In the title of Beltrami's article given on page 728, note 3, the g should be a capital in Geometria and the E lower case in non-euclidea. In note 4, page 725, nicht-Euklidischen should be nichteuklidischen. In note 1, page 727, nicht-Euklidische should be nicht-euklidische.

The final heading, VII., is Axioms of Geometry, under which it is said:

> The second controversy is that between the view that the axioms applicable to space are known only from experience, and the view that in some sense these axioms are given *a priori*.

Both these alternatives are wrong. These axioms are assumptions, belonging to what I have treated under the title "The Unverifiable Hypotheses of Science," in *The Monist*, October, 1910.

> The cruder forms of the *a priori* view have been made quite untenable by the modern mathematical discoveries. Geometers now profess ignorance in many respects of the exact axioms which apply to existent space, and it seems unlikely that a profound study of the question should thus obliterate *a priori* intuitions. . . . The enumeration of the axioms is simply the enumeration of the hypotheses of which some at least occur in each of the subsequent propositions.

On page 732, line 14, the comma after the word "however" is a misprint, and should be deleted.

Geometry with the assumption: Of any three points of a straight there is always one and only one which lies between the other two, Whitehead calls "descriptive geometry," a

horrible piece of nomenclature, which no one should adopt, since this name belongs to the system of Monge, 1794, for representing solids in a plane, though also used by Sylvester for a geometry excluding all notions of quantity, such as my "Synthetic Projective Geometry."

The article proceeds to

> the simplest statement of all. Descriptive Geometry is then conceived as the investigation of an undefined fundamental relation between three terms (points); and when the relation holds between three points *A, B, C,* the points are said to be "in the [linear] order *ABC.*"

O. Veblen's axioms and definitions, slightly modified, are as follows:

> 1. If the points *A, B, C* are in the order *ABC,* they are in the order *CBA.*

Dr. R. L. Moore (October 26, 1907) says this may be divided into parts, 1_1 inserting "distinct" before "points"; and 1_2 inserting "not all distinct," after "points."

> 2. If the points *A, B, C* are in the order *ABC,* they are not in the order *BCA.*
>
> 3. If the points *A, B, C* are in the order *ABC,* then *A* is distinct from *C.*
>
> 4. If *A* and *B* are any two distinct points, there exists a point *C* such that *A, B, C* are in the order *ABC.*

Dr. R. L. Moore modifies this to 4′ by inserting "different from *A* and from *B*," before "such." Then follow a definition, Def. 1, and axioms 5, 6, 7. Both in this definition, and in axiom 5 the shocking misprint occurs of using the symbol $\pm$, "plus or minus," for the symbol $\neq$, "is not equal to."

Dr. R. L. Moore had already in 1907 surprisingly simplified this set of assumptions by proving that 1_1 is a consequence of 2 and 5 and Def. 1, while 1_2 and 3 are both consequences of 2, 4′, 5, 6, 7 and Def. 1.[4]

> Lobachevski [or Bolyai] constructed the first explicit coherent theory of non-Euclidean geometry, and thus created a revolution in the philosophy of the subject. For many centuries the speculations of mathematicians on the foundations of geometry were almost confined to hopeless attempts to prove the "parallel axiom" without the introduction of some equivalent axiom.

[4] *Trans. Amer. Math. Soc.,* Vol. XIII., No. 1, pp. 74–76.

In the Bibliography, Whitehead says of Lobachevski:

> His first publication was at Kazan in 1826.

This is a mistake. In 1836 in his "Introduction to New Elements of Geometry," of which I was the first to publish a translation,[5] he says:

> Believing myself to have completely solved the difficult question, I wrote a paper on it in the year 1826: "Exposition succincte des principes de la Géométrie, avec une démonstration rigoureuse du théorème des parallèles," read February 12, 1826, in the séance of the physico-mathematic faculty of the University of Kazan, but nowhere printed.

No part of this French manuscript has ever been found. The latter half of the title is ominous. For centuries the world had been deluged with rigorous (!) demonstrations of the theorem of parallels.

Saccheri's book of 1733, containing a coherent treatise on non-euclidean geometry, of which I published the first translation, ended with another "demonstration rigoureuse du théorème des parallèles." If Saccheri had realized (as Father Hagen writes me he did) the pearl in his net, he could, with the new meaning, have retained his old title, Euclides ab omni naevo vindicatus, since the non-euclidean geometry is a perfect vindication and explanation of Euclid.

But Lobachevski's title is made wholly indefensible. A new geometry, founded on the contradictory opposite of the theorem of parallels, and so proving every demonstration of that theorem fallacious, could not very well pose under Lobachevski's old title. He himself never tells what he meant by it, never tries to explain it.

The title of Engel's book already given erroneously in the Bibliography under VI., is now, under VII., given again with the former and two additional errors.

After Riemann we see *Gesamte Werke* instead of gesammelte Werke.

In the title of Poncelet's work, on page 736, an accent is omitted which is given in the

[5] "Neomonic Series," Vol. V., 1897.

same title on page 676, where on the other hand the main word of the title is omitted.

The Beiträge of von Staudt appeared in two parts, the first in 1856, the second in 1860. How could Whitehead have made the mistake of calling this second part a "3rd ed." ?

George Bruce Halsted

Greeley, Colo.

Pearl and Jennings on Assortative Conjugation in the Protozoa

In general, the scientist's investigations receive the recognition they deserve from his fellow workers. This is true because the bulk of research consists in the working out of details in a scheme already stamped with authority. It is the unexpected, fundamentally new or truly brilliant result upon which the doctors disagree.

One of the best illustrations is a paper in *Biometrika* for February, 1907. In the demonstration of the existence of an assortative conjugation or homogamy in *Paramecium* analogous to the assortative mating previously found by Pearson in man, Pearl seemed to some of us to have struck a rich vein hitherto passed over by all prospectors. Others thought differently. Pearl's assays were discredited. In America, at least one review was declined. In England, J. J. Lister illustrated[1] by Pearl's paper his warning to biometricians to be sure they have a problem which is "sound from the standpoint of biology before bringing a formidable mathematical apparatus into action for its investigation."

Open criticism like that of Lister was more easily met[2] than the general indifference largely attributable to the *odium mathematicum*. This is now in a fair way to be overcome by the results being announced by Jennings. If these, in their turn, are being received by zoologists with but lukewarm enthusiasm, the fact indicates merely that the work is in advance of its time.

His recent study of conjugation in *Paramecium*[3] must be considered in comparison with Pearl's pioneer paper.[4]

a. Differentiation of Conjugants in Type and Variability.—The general belief that conjugants are on the average smaller than non-conjugants is quantitatively substantiated. In eleven "pure lines"[5] Jennings found conjugants to be from about 4 to nearly 14 per cent. smaller than the non-conjugants. In "wild" cultures, or in a mixture of differentiated pure lines, the mean for conjugants may be higher because only the large pure line is in conjugation. On the other hand, the conjugants may be abnormally small, 30 per cent. less than the non-conjugants, because only the smaller of the lines in the mixture is in conjugation.

Both absolutely and relatively, the conjugants are less variable than the non-conjugants. The difference in variability may be slight but generally it is large, for the conjugants are on an average about 33 per cent. less variable (relatively) than the non-conjugants.

The possible causes of this reduced variability are discussed. Lister's "Gametic Differentiation" is dismissed. Pearl's conclusion that equalization of individuals (undifferentiated or proconjugants) during the process of conjugation can not account for the lessened variability is confirmed. Jennings's conclusion, supported by abundant evidence, is that the low variability of conjugants is fully accounted for by the fact that conjugation does not occur till a certain growth stage has been reached, and does not occur in the largest individuals—the measurable variability of *Paramecium* being largely a growth phenomenon. Thus, the conjugants represent a definite and rather limited growth stage, the exclusion of both the larger and

[1] Lister, J. J., *Nature*, Vol. 74, pp. 584–585.

[2] Pearson, K., *Nature*, Vol. 74, pp. 465–466, 608–610, 635, 1907.

[3] Jennings, H. S., "Assortative Mating, Variability and Inheritance of Size in the Conjugation of *Paramecium*," *Journ. Exp. Zool.*, Vol. 11, pp. 1–134, July, 1911.

[4] Pearl, R., "A Biometrical Study of Conjugation in *Paramecium*," *Biometrika*, Vol. 5, pp. 213–297, 1907.

[5] The offspring of a single individual reproducing by fission has been called by Jennings a "pure line." In retaining the term here nothing more is implied than guaranteed purity of descent.

smaller individuals resulting in a pronounced reduction in variability. The growth factor may be in wild cultures supplemented by racial heterogeneity.

b. The Existence of an Assortative Mating.—Pearl's conclusion that there is a real assortative mating in *Paramecium* is backed up in every point by Jennings's more detailed study. Large individuals conjugate with large, small with small. Pearl's five series gave positive values of $r = .430$ to $r = .794$. Jennings's eight "wild" cultures of unknown racial composition all gave positive correlations of $r = .245$ to $r = .507$.

In twelve lots of conjugants from "pure races" with correlations ranging from $r = -.193$ to $r = .507$, there are ten positive and only two negative constants—both based on very small numbers and not statistically trustworthy with regard to their probable errors. The average of the twelve is $r = .251$. The correlation after separation of the pairs, is demonstrated to be greater than that determined on individuals which are united.

c. The Causes of the Assortative Mating.—Unquestionably, therefore, there is a real assortative conjugation. The redemonstration of the existence of a correlation between conjugants and the proof that it is not merely the result of heterogeneous cultures with only certain constituents in conjugation at one time is followed by a magnificent experimental-statistical analysis of the possible underlying factors. Equalization during mating, change of size during union, differential contraction due to killing fluid, environmental heterogeneity in the culture, are subjected to observation, experiment, measurement and statistical analysis. The specific results interest primarily the protozoologist. The broad and well-established conclusion is that the chief source of the correlation is, as Pearl five years ago maintained in *Biometrika,* a real assortative mating (larger individuals tending to conjugate with larger, smaller with smaller) arising in the fact that individuals must be of a certain degree of similarity in size to "fit."

d. The Evolutionary Significance of (*a*)–(*c*).—In connection with physiological differences resulting in different times of conjugation demonstrated by Jennings, the assortative conjugation is clearly a factor of the highest importance in the physiological isolation of the diverse races of *Paramecium.*

Apparently, the smaller size and lower variability of the conjugants have not a selective influence in maintaining the type of the population. The smaller extremes are, for the most part, younger than the conjugants, while the larger non-conjugants themselves conjugate after a few fissions. Moreover, the offspring of conjugants increase in size and in variability until they equal (or, in size exceed) the non-conjugants. Ex-conjugant offspring of individuals separated before completing conjugation.

Finally, the questions concerning the origin of heritable variations through conjugation within the "pure line," inextricably bound up as they are with the problem of the significance of conjugation, are reserved for a later paper. We are told, however, that heritable variations do, though rarely, arise as the result of conjugation within the "pure race."

Important as are the biological results of these two papers, their greatest value lies, not in concrete facts, but in their demonstration of the value of a widely applicable method. Let us hope they are only the beginning of the attack upon the many problems of morphology, physiology and genetics of the microorganisms which the biometric methods applied by Pearl and Jennings render possible.

J. ARTHUR HARRIS

"THE PASSING OF THE SLIME-MOULDS"

ON the first page of a late number of SCIENCE, April 13, appears the heading of a brief note entitled "The Passing of the Slime-moulds," by Professor C. E. Bessey. One who reads no farther, having in mind such titles as "The Passing of the Buffalo," "The Passing of the Chinese Empire," etc., might possibly infer that the slime-moulds were doomed

to speedy disappearance if not already gone. But such would be a serious error: the case is not so bad as that. These beautiful organisms were never more abundant than, nor did they ever receive such intelligent attention as, at the present moment. If the reader will leave the title-page and turn to the article itself he may, by reading a sentence or two, perhaps conclude that the "passing" in question is limited to the transit of thoughts about slime-moulds in the mind of our honored colleague during some thirty years. This is of course a matter of interest. We are glad to learn Professor Bessey's view even if inconstant, concerning any topic whatsoever. But we are still too hasty of conclusion. The "passing" intended is something different yet. Read to its conclusion the article in question would have us know that, in the judgment of the author, slime-moulds have finally passed from botany to zoology, have ceased to be plants (?) and have become animals—good riddance to them! Now in so much as our colleague esteemed does not at all trouble himself to define for us a plant or even an animal, the reader is left wondering; there rises the unbidden query: if we do not *know* what they are, why not let them alone, at least until we do know something definitive?

It appears, however, from the article, that DeBary, some half century ago, concluded the slime-moulds were not plants. It appears further that nothing has been added to De-Bary's argument until the recent appearance in the Encyclopædia Britannica of an article on the *Mycetozoa,* where these are set forth as animals for the mere reason that the author of the contribution so esteems them. Professor Bessey agrees. The whole thing is a matter of opinion; each of us, so far as stated reasons go, may think as he will;—and there you are!

But since the encyclopedia article has been forsooth the cause provocative in the present instance, it may be worth our while to note for a moment the inspiration of that particular essay, that we may better realize the full weight attaching to the opinion so consoling to our Nebraska author.

The writer of the 11th-edition article bears a distinguished name. He is a zoologist, a collaborator of Professor E. R. Lankester for whose "Treatise on Zoology," now appearing, the younger man wrote a chapter on the *Mycetozoa.* For the "passing of the slime moulds" so far, therefore, we are indebted to the English professor, and it is worth while reading the introduction to what is to be his masterpiece, no doubt, to his "Treatise," in order to completer information at first hand.

This introduction to have *raison d'être* at all must evidently show something new. It is fair to state that the only novelty discoverable, aside from a multitude of unnecessary and hybrid terms, consists in the amount of botany offered, both in the introduction and in volume one. In the introduction the independent life of the green plant is emphasized at length, as matter heretofore strange to the zoological reader. The "subtle process" by which green plants take up N in the form of *ammonia* will come as information to botanists as well; while the statement that the animal depends for food upon "hydrocarbons" ought to be news even to zoologists.

That the earliest plants are to be traced to flagellate colony-building protozoa, similar to the *Volvocineæ*—here reckoned animals, of course—is also a contribution to botany deserving grateful recognition. But it is admitted that certain organisms "devoid" of chlorophyl are plants. Here belong "fungi, bacteria and a few others."

> Autonomous fungi and the saprophytes depend for their food on the products supplied to them by the chlorophyl-holding cells of green plants.

Finally, the summary of zoological wisdom in this remarkable discussion appears in the following elegant sentence:

> The colorless or greenless plants are descended from green chlorophylligerous ancestors: mouthless, gutless animals are descended from mouth-bearing, gut-hollow animals.

Now as above stated, the encyclopedia man prepared a chapter on Mycetozoa to follow in Vol. I. this erudite introduction. The slime-moulds are protozoa and come in for the present, until better accommodations are at

hand, between the *Heliozoa* and the *Radiolaria*. This, of course, need make little difference. The slime-moulds are "mouthless and gutless" and should go anywhere with that set. From just which, however, of the "mouth-bearing, gut-hollow" creatures such graceful forms as *Arcyria* and *Lamproderma* have descended will no doubt be made clear on some future page!

Such is the sort and kind of finished scholarship with which it is now sought to align American botanists.

But really does it much matter where these things are placed? For 150 years they have been handled by the botanist. If they are now to go to the zoologist, or the chemist, he must show some reason for his claim. Some day, refined research, perhaps by methods not now devised, will show more clearly lines of descent and so of genetic kinship. That day is not here yet—so far, at least, as is to be learned from authorities herein cited.

Meantime, it may be said in conclusion, the great collections on which the Oxford professor and his pupils have so gratuitously toiled, still occupy probably an honored alcove in the *herbarium* of the British Museum.

T. H. MACBRIDE

IOWA CITY,
April 13, 1912

SCIENTIFIC BOOKS

Meteorology, a Text-book on the Weather, the Causes of its Changes and Weather Forecasting, for the Student and General Reader. By WILLIS ISBISTER MILHAM. The Macmillan Company. 1912.

As the author of this latest treatise on meteorology is a most popular professor of astronomy in Williams College it is natural that this book shall bear all the characteristics of an admirable text-book for class use. It is also intended for the general reader. It starts at the beginning and must be intelligible to all, but it has abundant references to current literature for the use of those who wish to study further. The author has not attempted the history of the science, nor the relations of climate to disease or plants, nor has he laden his pages with heavy mathematical work nor with a dozen other special items that would be included in an encyclopedic treatise. Professor Milham's book is simply an enlargement of the lectures which he has been giving for the last eight years in Williams College which aims to give its students a broad education in languages and sciences that have to do with our every-day life. The genial generosity of the author is shown by his painstaking acknowledgment of every authority from whom he quotes and one might imagine the book to be a compilation were it not for the many good ideas originating with the author. As a popular text-book it is admirable and fills a want distinct from that which is satisfied by the excellent work of Davis. After four hundred pages devoted to the atmosphere and the weather bureaus the author adds a hundred pages, as part two, devoted to the climate, the floods, electric, optic and acoustic phenomena.

The great utility of reliable forecasts has undoubtedly always been an incentive to all mankind and throughout all ages to apply our crude scientific knowledge to the study of the atmosphere, but since the days of Galileo the love of knowledge for its own sake—the love of research into the hidden things of nature has been a characteristic of civilized man. The conflict between darkness and light, the contest between superstition and intelligence, the fight between conservatism and progress has nowhere been so persistent as during the past forty years and in the field of meteorology. Professor Milham is quite correct in saying that there is no subject wherein ignorance and superstition are more nearly universal than in connection with the weather. Perhaps we can not blame the well-educated citizens for a certain amount of ignorance since so little is taught about meteorology either in high schools or colleges. Apparently another century must elapse before courses of laboratory experimentation have been devised for use in our higher schools of science.

Our subject begins with the constitution of the atmosphere considered as a mixture of several gases and vapors; these are warmed by

the sunshine and cooled by the radiation of heat; the upper layers of the atmosphere have both heat and moisture carried upward into them by convection, namely, by air that ascends from near the earth's surface: on the other hand the lower atmosphere is slightly cooled by the descent of an equivalent mass of the upper air. Of course radiation of heat from the atmosphere and the earth outwards goes on continuously, whereas convection is spasmodic; hence, the distribution of temperature is a regularity that is modified by many irregularities. The annual and the diurnal periods and the irregular variations of heat are appreciated by every one as is also its diminution with altitude. The barometric gradient is generally regarded as the cause of the motions of the atmosphere, but the rotation of the earth on its axis modifies these winds in such a way that northerly winds become northeast and southerly winds become southwest and so on around the circle. Of course the atmospheric pressure has its regular annual and diurnal variations and its irregular ones also, as well as its diminution with altitude. The regions of high pressure on the earth's surface are also regions of descending dry and dense air, whereas the regions of low pressure are those toward which the air is blowing and are the ordinary storm centers. The general circulation of the atmosphere is maintained by such barometric differences far above the earth's surface as well as at sea level: these are complicated with the large differences of the highs and lows of the barometric pressure that maintain our storm areas. The paths of these areas are shown by Milham in small maps prepared by several different persons—Bigelow, Van Cleef, Bebber, Loomis, Russell. The attempt to predict the path of any area of high or low pressure is the fundamentally difficult problem of the forecaster and is one to which considerable attention is given in Professor Milham's book. The volume is illustrated by 157 illustrations and fifty charts and a number of numerical tables. A brief summary shows that Milham's meteorology is a work destined to be eminently useful, both to the teacher and to the general reader. The publishers have spared neither money nor pains in order to make this beautiful volume acceptable to the author, the teacher and the student.

C. A.

Probleme der physiologischen und pathologischen Chemie. Fünfzig Vorlesungen über neuere Ergebnisse und Richtungslinien der Forschung für Studierende, Aerzte, Biologen und Chemiker, von Dr. Otto von Fürth, A. Ö. Professor für angewandte medizinische Chemie an der Wiener Universität. I. Band, Gewebschemie. Leipzig, Verlag von F. C. W. Vogel. 1912. Pp. xv + 634.

This is neither a systematic text-book, an encyclopædic compilation of facts, nor a mere epitome of current theories or methods—it is quite different from other recent publications in this field of science. Physiological chemistry has been the subject of an exceptionally large number of useful handbooks and comprehensive monographs on special topics, all published in very recent times and useful as works of reference. Here at length is a book which can not only be "tasted" or "swallowed," but even "chewed and digested," according to the Baconian classification. Assuming a familiarity with at least the elements of biochemistry on the part of every reader and making no pretense to cover the field with uniform completeness or comprehensiveness in every chapter, Professor v. Fürth has furnished a readable volume of refreshing novelty.

We are in danger, in present-day science, of becoming the slaves of system. These new lectures break away from some of the current tradition in respect to form and appeal to him who is not so much in search of organized literature as of inspiring viewpoints. The justification for this series of essays on the accomplishments and tendencies of the chemical physiology of these days—essays unhampered by the conventional text-book requirements—is well expressed by the author.

Ich vermag mich niemals eines Gefühles von Neid zu erwehren wenn ich zum Beispiel Briefe

von *Liebig, Wöhler* oder *Berzelius* lese und sehe, wie für diese Glücklichen jede wissenschaftliche Publikation ein Ereignis war. Mit welcher Liebe wurde alles, auch wenn es nur wissenschaftliche Kleinarbeit war, aufgenommen, mit welcher Freude wieder gelesen und überdacht. Wir laufen Gefahr, durch die Masse literarischer Produktion schliesslich das naive Vergnügen am Neuen einzubüssen und der Neugierde verlustig zu werden, die jedes ursprüngliche Individuum, ob jung oder alt, dem Unbekanntem entgegenbringt und die schliesslich die Seele jeder echten Naturforschung ist. Und wenn heute in einem Fache, wie es die *Biochemie* ist, der Fachmann im Schweisse seines Angesichtes eben noch imstande ist, sich über die Literatur in ihren wichtigsten Erscheinungen einigermassen zu orientieren, ist dies für den Fernerstehenden bereits längst ein Ding der Unmöglichkeit geworden.

The titles of the individual lectures are too diverse to be quoted here in detail. The treatment throughout is animated by the viewpoint of the physiologist, rather than the chemist. Chemistry is made to elucidate biological functions. The lectures can not fail to interest pathologist, therapeutist and surgeon as well as physiologist; for their problems are considered in common in a truly biological spirit well exemplified in a chapter on the thyroid. In a chapter on the kidneys, for example, the theories of secretion, the problems of uremia, the inadequate status of our knowledge of eclampsia, transplantation experiments, etc., are interwoven into suggestive relationships reminding one of how much still awaits solution. The critique of the author obviously can not always be equally forceful or correct; how trenchant it may at times become is illustrated by v. Fürth's comments in relation to eclampsia:

Für den Begriff eines "urotoxischen Koeffizienten," so schön und gelehrt dieser Name auch klingen mag, vermögen wir seinem Erfinder mit bestem Willen nicht dankbar zu sein.

It is unnecessary to proceed far in these lectures to discover that the author is something more than a compiler. He has studied physiological problems in the laboratory; and, as might be expected, the expert familiarity with certain phases of the subject betrays itself advantageously in many places. For this reason, too, not all of the topics are treated with equal success. It is as gratifying as it is uncommon to find, in a European book, a thorough appreciation of the work of American investigators along physiological lines.

To one who enjoys the historical aspects of science the lectures will afford many entertaining reminders. There is a "subjective" tendency manifest throughout, which occasionally goes to the extreme of hyper-appreciation. The personal factor and individuality of the treatment crop out frequently in delightful "touches," a few of which may be quoted here. Speaking of the prolongation of life and the "sour milk" therapy of Metchnikoff:

Ist es doch das gute Recht der Menschheit, sich des grossen Allbezwingers mit allen ihr zugänglichen Mitteln zu erwehren, und es wäre sicherlich sehr erfreulich, wenn ein so einfaches Pharmakon, wie vergohrene Milch sich ihr als wirksames Kampfmittel erweisen sollte (p. 44).

For intermediary metabolism, we read,

passt ein Bild, das einst ein geistvoller Mathematiker auf die Integralrechnung angewandt hat, um die unergründlichen Tiefen derselben seinen Hörern begreiflich zu machen. Er sagt, sie gleiche einem Meere, an dessen Ufern der Fischer immerhin das sammeln mag, was aus der Tiefe nach der Oberfläche dringt; doch vermag keines Menschen Auge die ganze Daseinsfülle zu ergründen, welche die abyssischen Tiefen erfüllt (p. 46).

Note the attitude toward exploded theories:

Es dürfte aber vermutlich noch eine ganze Weile dauern, bis die Uroleucinsäure aus der physiologischen Chemie verschwunden sein wird; denn die Biochemiker zeichnen sich meist durch eine besondere Pietät gegenüber den Irrtümern ihrer Vorgänger aus. In der "reinen" Chemie pflegt man kürzeren Prozess zu machen und hält es nicht für nötig, in jeder Abhandlung die ganze Kette überwundener Irrtümer früherer Generationen von neuem abzuhaspeln (p. 52),

or again:

Es ist stets eine missliche Sache, wenn man über unklare Dinge eine klare Auskunft geben soll (p. 67).

Commenting on the superiority of certain

synthetic over the older natural products, he writes:

Ich fürchte fast, es würde manchem Teilstücke der Herrlichkeit klassischen Altertums ebenso ergehen, wenn man es mit der Exaktheit einer chemischen Synthese vor unseren Augen neu erstehen lassen könnte. Von dem alten Märchenglanze würde vielleicht nicht allzuviel übrig bleiben (p. 76).

The future possibilities of protein synthesis are introduced with these words:

Sowohl für junge Menschen als auch für junge Wissenschaften gehören Ideale zu den notwendigen und gesunden Lebenselementen, da ihnen die Fähigkeit innewohnt, latente Kräfte zu mobilisieren und nützlichen Zielen dienstbar zu machen, wenn auch die Unerreichbarkeit strenge genommen zum Begriffe eines Ideales mit dazu gehört (p. 96),

whereas the older attempts are dismissed thus:

Dass wir auf diesem und auf ähnlichen Wegen zu wirklichem echten Eiweiss gelangen Könnten, ist wohl nicht viel wahrscheinlicher, als wenn jemand einen Haufen Lettern in einem Sacke durcheinander mischen, sodann auf den Tisch ausschütten und nun hoffen möchte, dass dieselben sich zu einem schönen Gedichte gruppieren würden (p. 97).

That there is no attempt to hide our ignorance in certain fields is indicated in connection with internal secretion,

ein viel missbrauchtes Schlagwort, welches im Laufe des letzten Dezenniums zu einer gewaltigen Popularität gelangt ist. Dasselbe bezieht sich auf die Funktion einiger Organe, deren physiologische Rolle und Bedeutung, ungeachtet eines grossen Aufwandes von Mühe und Arbeit, in tiefes Dunkel gehüllt ist. "Denn eben wo Begriffe fehlen, da stellt ein Wort zur rechten Zeit sich ein;"—so sprach einmal ein weiser Mann, der zwar von "inneren Sekretionen" noch nichts ahnte, dafür aber über manche andere Dinge um so besser Bescheid wusste (p. 404).

The sources of the literature are indicated in a comprehensive way and the progress of research has evidently been followed up to very recent months. There is, as a rule, no irritating wealth of details; yet the essential steps in important reactions, such as the synthesis of suprarenin, are reviewed with painstaking accuracy. A second volume on metabolism is promised soon.

The publication of the lectures was the outcome, the author tells us, of a desire

meine eigene Freude an biochemischem Suchen und Erkennen anderen, die danach Verlangen tragen, zu übermitteln und auf diesem Wege meiner Wissenschaft zu dienen.

In this Professor v. Fürth has succeeded.

Lafayette B. Mendel
Yale University

The Lower Cretaceous Deposits of Maryland. By William Bullock Clark, Arthur B. Bibbins, Edward W. Berry and Richard S. Lull. Maryland Geological Survey. 1911. Pp. 1–622, pl. I.–XCVII.

This volume, the fourth of a series of exhaustive reports dealing with the systematic geology and paleontology of Maryland, is devoted to the Lower Cretaceous and is a distinct and invaluable contribution to the history of the Atlantic coastal plain geology of which the Maryland section may be taken as the type. The first hundred pages are devoted to the coastal plain region of the state and comprise a description of its physiographic features and a briefer statement of the Cretaceous, Tertiary and Quaternary formations, followed by a more detailed account of the areal distribution, lithologic character, stratigraphic and structural relations and organic remains of the three Lower Cretaceous formations involved. These, according to the nomenclature adopted, are, in the order of their superposition, the Patuxent, the Arundel and the Patapsco formations, together comprising the Potomac group. An especially valuable part of this portion of the volume is the historical review of opinion and the bibliography, for, be it known, the geology of Maryland has been the subject of much discussion and difference of interpretation. For example, the Patuxent and Arundel formations, on the basis of their contained vertebrates, were held by Marsh to be of Jurassic age, while the abundant plant-remains argued indisputably for their Lower Cretaceous age, a conclusion to which Professor Lull, who has reexamined the vertebrate material, has also come.

The paleontology fills the remaining five sixths of the volume and comprises reports on the Molluska by Clark, the Vertebrata by Lull and the Plants by Berry. The molluscs are unimportant, consisting of only five species, all new, four of which are from the Arundel and one—a *Unio*—from the Patapsco. They are of little or no value in correlation.

The vertebrate fauna—found only in the Arundel—is much larger and consists of seven genera of dinosaurs, a crocodile, at least one turtle, and a ganoid fish. In discussing the affinities of the dinosaurs Professor Lull compares them with the Morrison of the west and the Wealden and Jurassic of Europe, concluding that "the weight of evidence would seem to place this fauna beyond the Jurassic into the beginning of Cretaceous times." As the Arundel reptiles are closely related to, if not in some cases actually identical with, those from the Morrison of Colorado, their bearing on the age of the latter is of importance.

The major portion of the volume is devoted to the elaboration of the fossil flora by Mr. Berry. As a preliminary to the correlation of the Potomac flora he devotes a chapter to the "Lower Cretaceous Floras of the World," in which is reviewed the plants of this age in Switzerland, Portugal, France, England, Belgium, Germany, Saxony, Africa, Peru, Japan, China and Spitzbergen, as well as the several approximately contemporaneous deposits in this country. Complete lists of the species occurring at each locality are given, and while no attempt has been made to revise the nomenclature or determinations, they furnish an exceeding valuable basis for correlative and stratigraphic comparisons. Following this is a chapter on the "Correlation of the Potomac Formations." In this it is shown that the Patuxent and Arundel formations on the basis of their contained floras are essentially a unit of early Cretaceous age whose affinities all lie with the floras which preceded them, while the Patapsco, which was separated by a long time interval, has its strongest relations with the floras which followed it. The conclusion is reached that the Patuxent and Arundel formations "considered as a unit represent all except possibly the earliest part of the Neocomian and all of the Barremian of standard European sections." In this country it is believed to correspond in whole or in part with the Trinity of Texas, the Lakota of the Black Hills, the Kootenai of the Rocky Mountain region and the Cretaceous portion of the Knoxville of the Pacific Coast. The Patapsco is correlated with the European Albian and with the Fuson of the Black Hills. It is chiefly remarkable for the fact that it appears to have witnessed the introduction of the dicotyledons.

Over four hundred pages and seventy-five plates are devoted to the elaboration of the flora, which, according to Mr. Berry, comprises 144 species distributed among the several groups as follows: Filicales 43, Lycopodiales 1, Equisetales 2, Cycadophyta 33, Ginkgoales 1, Coniferales 29, Angiospermæ 25, of unknown systematic position 10. The treatment throughout has been conservative and rational, and has been based on all of the original material as well as a vast amount of recently and carefully collected material, with the result that the number of recognized species has been greatly reduced. Thus, Fontaine referred 42 species of ferns to the living genus *Thysopteris,* but these have been reduced to 5 and all referred to *Onychiopsis.* The number of species of *Cladophlebis* has been much reduced, as they have in the genera *Abietites, Sphenolepis, Arthrotaxopsis,* and especially *Sequoia.* A new generic type of fern, thought possibly to be referable to the Matoniaceæ, has been established for certain fronds of medium or large size and pseudodichotomous habit. Another fern, originally referred to *Baieropsis,* is shown clearly to be a member of the Schizæaceæ.

However, most biologic interest centers in the angiosperms, since they are as old as any of which we have definite knowledge. From the original descriptions and roughly prepared figures, it appeared that they were crude and archaic as befitted their first appearance, but with newer and better material and improved methods of reproducing them, they are seen to

be highly organized and surprisingly "modern" in aspect, thus showing that our knowledge of the actual starting point for this now dominant group is still imperfect. Mention may be made of a few of the more interesting forms. A small *Sagittaria*-like leaf is described under the new generic name of *Alismaphyllum*. What appears to be a fruiting sedge is included under *Cyperacites*, while under the name of *Plantaginopsis* is figured a plantain-like leaf and fruit possibly belonging to the Xyridales, which completes the list of monocotyledons. The dicotyledons are included under *Populus*, *Populophyllum*, *Nelumbites* (a very *Nelumbo*-like leaf formerly referred to *Menispermites*), *Sapindopsis*—the most abundant and important dicotyledonous plant of the time—*Celastrophyllum*, a form-genus suggesting the Celastraceæ, *Sassafras*, quite closely approximating the living form, *Araliæphyllum* and *Ficophyllum*, form-genera recalling *Aralia* and *Ficus* respectively, and a few others that are without very clearly understood living affinities. Altogether, the elaboration of the Lower Cretaceous floras of Maryland is of a high order, and Mr. Berry is to be congratulated on the completion and publication of the work which must long remain as a model of its class.

F. H. Knowlton

Woodland Idyls. By W. S. Blatchley. Indianapolis, Ind., The Nature Publishing Co. 1912. Pp. 242.

Mr. Blatchley has again published a nature book, interesting, instructive, enjoyable. Just the kind of a book to take out on a summer vacation to impart the love of nature and her creatures and teach one the value of simple things. It is a chronicle of several vacations spent in the fields and woods, camping at night in a tent, by day fishing, watching birds and insects, and taking notes on the happenings around him. Mr. Blatchley is qualified to speak knowingly and scientifically of nature's secrets, by long years of investigation in various phases of zoology and botany. The specialist will find here many little notes on the habits of birds, fish, turtles, small mammals and insects fresh from the mind of a careful observer. Like a clear, sweet, woodland brook, there runs through all a philosophic, yet optimistic strain of adaptation of human needs to the simplicity of nature.

N. Banks

The Evolution of Animal Intelligence. By S. J. Holmes. New York, Henry Holt. 1911. Pp. iii + 296. $2.75.

Professor Holmes gives a rather popular presentation of some of the recent work in animal behavior. He does not pretend to make his treatment of the field of behavior at all complete. The subjects he treats at some length are as follows: tropisms; behavior of protozoa; instincts and their origin; pleasure, pain and the beginnings of intelligence; types of intelligence in crustaceans, mollusks, insects, lower vertebrates and mammals. The final chapter is devoted to the study of the mental life of apes and monkeys.

While most if not all of these subjects have received more skilful treatment in the hands of Jennings, Mast, Washburn, Yerkes and Thorndike, Holmes gives a readable presentation of certain phases of behavior which will be of service to students beginning the study of comparative psychology. The book's value lies in the readiness with which it lends itself to pedagogical purposes.

John B. Watson
Johns Hopkins University

Butterfly Hunting in Many Lands: Notes of a Field Naturalist. By Geo. B. Longstaff, M.A., M.D., Oxon. Longmans, Green & Co. 1912. Pp. xviii + 724, with sixteen plates, seven colored. Price, $7.00 net.

The author describes this work as "an attempt, possibly a foolish one, to put into a readable form the technical diaries of a wandering entomologist, and to entomologists alone it appeals." The reviewer is inclined to agree. After a chapter on early reminiscences, Dr. Longstaff devotes nearly five hundred pages to notes on his captures from 1903 to 1910, during which time he visited Canada, certain of the West Indies, Panama,

northern South America, South Africa, Algeria, Egypt and the Soudan, India and Ceylon, China, Japan, New Zealand and Australia. He very evidently collected vigorously, both by eye and by net, not confining himself to butterflies, notwithstanding the title. In fact the majority of the illustrations in this part of the book as well as many of the notes concern other insects. These notes are largely simple records of captures, leavened somewhat by random remarks concerning them or his traveling experiences. In view of the large amount of ground covered in so short a time, the lists of species for given localities are naturally too incomplete to be important and they must certainly detract from the interest of the narrative for non-entomologists.

The last chapter is based upon two papers by the same author in the *Trans. Ent. Soc. London* and is a summary of bionomic notes made chiefly by Dixey and the author on butterflies. The odor of many species is described; mutilated specimens are listed as having escaped from foes; peculiarities of flight and resting attitudes, including the selection of harmonizing backgrounds, are discussed, and the conclusions are orthodox neo-Darwinian.

The appendix consists of a translation of twelve of Fritz Müller's papers on the scent-organs of Lepidoptera. Six of these were published in Portuguese in the *Arch. do Mus. Nat. do Rio de Janeiro* where they have been inaccessible to many. The translations are by E. A. Elliott and the introduction to the appendix is by Poulton. The collection and translation of these papers will be a great help to students and it is well that they be read in connection with Longstaff's observations. However only the last chapter (and not all of that) is necessary for this purpose, and it does seem unfortunate that the rest of the book was not bound separately.

F. E. LUTZ

THE TALKING DOG

EXTENSIVE comment has been made in the German and even in the American daily press on the reported conversational ability of "Don," a German setter seven years old, belonging to the royal gamewarden Ebers at Theerhutte in Gardelegen. Numerous observers reported that he had a vocabulary consisting of eight words, which he could speak if food were held before him and the following questions propounded: "Was heisst du?" "*Don.*" "Was hast du?" "*Hunger.*" "Was willst du?" "*Haben haben.*" "Was ist das?" "*Kuchen.*" "Was bittest du dir aus?" "*Ruhe.*" Moreover, he was said to answer categorical questions by "*Ja*" and "*Nein*"; and in reply to another question, to speak the name, "*Haberland.*" Among others whom popular report mentioned as witnesses to this extraordinary ability of the dog was Mr. Oskar Pfungst, of the Psychological Institute of the University of Berlin, whose important tests on the horse of Herr von Osten, "Der Kluge Hans," have lately been published in English.[1] Mr. Pfungst had in fact investigated the behavior of the dog in collaboration with Professor Vosseler and Dr. Erich Fischer, keeping detailed memoranda on the tests, and making a number of phonographic records. Partly to clear up misapprehension of his own position and partly for the enlightenment of the serious general public, he gave out a brief popular report of his work,[2] a summary of which appears below.

Having proposed three definitions of speech: first, properly, as the use of vocal sounds to convey to the listener an idea experienced by the speaker; secondly, more loosely, as the production of vocal sounds learned by imitation, but used without knowledge of their meaning to the hearer; and thirdly, as the production of vocal sounds not imitative of human speech, having no meaning to the speaker, but producing in the hearer illusions of definitely articulated, spoken words, uttered to convey meaning—Mr. Pfungst then asks to

[1] Pfungst, Oskar, "Clever Hans." Translated by Carl L. Rahn. New York, Henry Holt & Company, 1911.

[2] "Der sprechende Hund," von Oskar Pfungst (Berlin), Sechste Beilage zur Possischen Zeitung, 27 April, 1911.

which class the speech of Don properly may be referred.

First, it is plain enough that the dog does not use words with any consciousness of their meaning to the hearer. His vocabulary is always given in order, beginning with "*Don*" and ending with "*Ruhe.*" If the order of questioning is varied he is called "*Kuchen*" and he desires "*Hunger,*" etc. (Here it may be noted that the author was unable to get even approximations to the last three words in the list accredited to the animal.)

Secondly, it is evident, says Mr. Pfungst, that he is not using words learned by imitation. The author assumes that any imitator of another speaker would vary the pitch, intensity or accent of his words as the imitatee's were varied. Don's voice—a high tenor, ranging from F on the bass clef to the octave above middle c, usually pitched in talking near d above middle c—is not varied when the pitch of the questioner's voice is altered. Furthermore he does not imitate changes in accent or intensity. He is as likely to say "*Kuchén*" as "*Kúchen*"; "*Hungér*" as "*Húnger,*" etc. From the legitimacy of the author's adoption of this criterion, however, the reviewer is inclined to dissent. His own experience with a child of two and one half years, learning readily to speak a large number of words and phrases from imitation, and able to give both vowel and consonant values with perfect distinctness, for several months was that she would not imitate changes of intensity or pitch, although she usually showed apparent willingness to try. To apply this principle in the case of the dog would require the assumption of an attentive ability as well as of motor skill, far in excess of any of which that animal has given evidence. But Mr. Pfungst offers other disproof of the imitation hypothesis which to the reviewer seems adequate. This is found in the method of learning. The first word which the dog is reported to have uttered is "*Haben.*" We are assured that being asked, "Willst du etwas haben," he thereupon pronounced distinctly the words, "*Haben haben haben,*" and was rewarded with food for his pains. When he afterwards attempted to pronounce the words he would give many inarticulate gurgles, but the food was given only when the correct number of syllables were uttered at once. The owner's family state that ten repetitions, some a week apart, sufficed for this learning. The word, "*Ruhe,*" was first uttered after a command, "Ruhe," by the owner's daughter. Hearing the dog's response, she demanded, "Was sagst du da," and obtained again the answer "*Ruhe.*" He was then taught to give this word after his fifth question, "Was bittest du dir aus?" The name, "*Haberland,*" which none of the investigators could obtain from him, was first answered without instruction to the question, "Wer hat den ersten Artikel über dich in die Zeitung gebracht?" These facts are hardly consistent with any provable experience in learning by imitation. Indeed, it may be remarked that to the reviewer, who has spent the greater part of two years in experimentation on the behavior of dogs under controlled conditions, the animals' vagueness of perception and extremely low degree of attention would make a very strong presumption against the possibility of their learning even the simplest acts by "observation and imitation."

Mr. Pfungst concludes that the speech of Don is therefore to be regarded properly as the production of vocal sounds which produce illusions in the hearer. He calls attention to the fact that not even the number of syllables in any given "word" of Don's is constant. The dog makes only one vowel sound, having a value lying between *o* and *u*, varying considerably, but usually nearer *u*. The experimenters could not hear from him certainly either *a* or *e*. His one guttural-aspirant is like the German *ch*, and does duty for *k* and *h*. There is also a nasal, of a value lying between *n* and *ng*. When it is not prolonged it passes for a *d*, as in "*Don.*" He really never makes the sound of *b*, *d*, *k*, *l* or *r*. When he utters a word expressed by [*(ch)unguo*], not much effort is required from a suggestible hearer to perceive the sound as "*Hunger.*" When in making phonograph records the questioner asked merely "Was?," the dog gave

the customary answers, *"Don," "Hunger?" "Haben haben," "Kuchen,"* etc., of which however only two out of sixteen answers were intelligible. Of 168 answers preserved on phonograph records, 71 per cent. were disyllabic and of the monosyllabic noises 68 per cent. were given when a considerable pause had elapsed between the last answer and this question. The "answers" were really incorrect fully as often as otherwise. Disinterested hearers could seldom distinguish his *"Hunger"* from his *"Haben,"* nor his *"Ruhe"* from his *"Kuchen,"* etc. It was as easy for others to perceive some of these same sounds as *"Engelhopf"* or *"Hallelujah"; "Huhn"* or *"Honig."* Here it seems to the author we have a case quite parallel with our common interpretation of the night-swallow's call as "Whip-poor-Will" when in fact the sounds are nearly "Pfif-ah-rih"; and with the common German interpretation of their *Steinkanz's "Kuwitt"* or *"Kuwiff"* as *"Komm mit,"* thus making him in popular superstition the messenger of death. But for a strong and uninhibited tendency thus to "apperceive" them, neither these calls nor the "words" of Don would be taken as other than meaningless noises.

On psychological grounds, Mr. Pfungst concludes, the explanation is comparatively simple; the uncritical do not make the effort to discriminate between what is actually given in perception and what is merely associated imagery, which otherwise gives to the perception a meaning wholly unwarranted; and they habitually ignore the important part which suggestion always plays in ordinary situations.

Accepting this explanation as satisfactory we may expect the majority of animal lovers to continue to read their own mental processes into the behavior of their pets. Nor need we be astonished if even scientists of a certain class continue at intervals to proclaim that they have completely demonstrated the presence in lower animals of "intelligent imitation" and of other extremely complicated mental processes—inferred from the results of brief and lamentably superficial tests, and published as proven facts without further reflection.

HARRY MILES JOHNSON
THE JOHNS HOPKINS UNIVERSITY

FOURTH LIST OF GENERIC NAMES FOR THE "OFFICIAL LIST OF ZOOLOGICAL NAMES," PROVIDED FOR BY THE GRAZ CONGRESS

15.[1] The following generic names of Diptera are proposed for *inclusion* in the "Official List of Generic Names." The species mentioned are the correct types, according to Coquillett, 1910.

Anopheles Meig., 1818, 10, type *bifurcatus.*
Anthomyia Meig., 1803, 281, type *Musca pluvialis.*
Chrysops Meig., 1800, 23, type *cæcutiens.*
Corethra Meig., 1803, 260, type *Tipula culiciformis.*
Culex Linn., 1758a, 602, type *pipiens.*
Cuterebra Clark, 1815, 70 type *Œstrus cuniculi.*
Gasterophilus Leach, 1817, 2, type *Œstrus intestinalis* (cf. *Œ. equi*).
Hæmatobia St. Farg. & Serv., 1828, 499, type *Conops irritans.*
Hippelates Loew, 1863, 36, type *plebejus.*
Hippobosca Linn., 1758a, 607, type *equina.*
Hypoderma Latr., 1818, 272, type *Œstrus bovis.*
Lucilia Desv., 1830, 452, type *Musca cæsar.*
Musca Linn., 1758a, 589, type *domestica.*
Muscina Desv., 1830, 406, type *stabulans.*
Nycteribia Latr., 1796, 176, type *Pediculus vespertilionis.*
Œstrus Linn., 1758a, 584, type *ovis.*
Ophyra Desv., 1830, 516, type *Anthomyia leucostoma.*
Phora Latr., 1796, 169, type *Musca aterrima.*
Piophila Fall., 1810, 20, type *Musca casei.*
Psorophora Desv., 1827, 412, type *Culex ciliatus.*
Sarcophaga Meig., 1826, 14, type *Musca carnaria.*
Stegomyia Theob., 1901, 234, type *Culex calopus.*
Stomoxys Geoffr., 1762, 538, type *Conops calcitrans.*
Tabanus Linn., 1758a, 601, type *bovinus.*
Tipula Linn., 1758a, 585, type *oleracea.*

16. The following generic names of Diptera are proposed for *exclusion* from the "Official List," on the ground that they are absolute homonyms and preoccupied.

Acanthina Wiedem., 1830, not Fisch., 1806.
Allocotus Loew, 1872, not Mayr, 1864.
Ammobates Stann., 1831, not Latr., 1809.

[1] Paragraphs are numbered continuously with the earlier lists.

Anepsius Loew, 1857, not LeConte, 1852.
Anoplomerus Rond., 1856, not Latr., 1844.
Archilestes Schin., 1866, not Selys, 1862.
Ascia Meig., 1822, not Scop., 1777.
Aspilota Loew, 1873, not Fœrst., 1862.
Asthenia Westw., 1842, not Hübn., 1816.
Astoma Lioy, 1864, not Oken, 1815.
Atomaria Bigot, 1854, not Steph., 1830.
Atrichia Loew, 1866, not Schrank, 1803.
Blacodes Loew, 1874, not Dej., 1859.
Blax Loew, 1872, not Thom., 1860.
Brachygaster Meig., 1826, not Leach, 1817.
Callopistria Loew, 1873, not Hübn., 1816.
Centor Loew, 1866, not Schönh., 1847.
Ceria Fabr., 1794, not Scop., 1763.
Chauna Loew, 1847, not Illig., 1811.
Chrysonotus Loew, 1855, not Swains., 1837.
Clytia Desv., 1830, not Lam., 1812.
Coprina Zetters., 1837, not Desv., 1830.
Coquillettia Willist., 1896, not Uhler, 1890.
Cyrtosoma Brauer & Bergenst., 1891, not Walk., 1829.
Dendrophila Lioy, 1864, not Swains., 1837.
Diabasis Macq., 1834, not Hoffmanns., 1819.
Diphysa Macq., 1838, not Blainv., 1834.
Discocephala Macq., 1838, not Lap., 1832.
Empheria Winn., 1863, not Hag., 1856.
Enicopus Walk., 1833, not Steph., 1830.
Erichsonia Desv., 1863, not Westw., 1849.
Eriogaster Macq., 1838, not Germ., 1811.
Eristicus Loew, 1848, not Wesm., 1844.
Eudora Desv., 1863, not Less., 1809.
Eumetopia Macq., 1847, not Westw., 1837.
Eumetopia Brauer & Bergenst., 1889, not Westw., 1837.
Euphoria Desv., 1863, not Burm., 1842.
Eurycephala Röd., 1881, not Lap., 1833.
Exocheila Rond., 1868, not Rond., 1857.
Fabricia Meig., 1838, not Blainv., 1828.
Fallenia Meig., 1838, not Meig., 1820.
Grassia Theob., 1902, not Fisch, 1885.
Halithea Hal., 1838, not Savig., 1817.
Helobia St. Farg., & Serv., 1828, not Steph., 1827.
Heteroneura Fall., 1823, not Fall., 1810.
Heterostoma Rond., 1856, not Hart., 1843.
Himantostoma Loew, 1863, not Ag., 1862.
Hydrochus Fall., 1823, not Germ., 1817.
Hyria Desv., 1863, not Lam., 1819.
Icaria Schin., 1868, not Sauss., 1853.
Idiotypa Loew, 1873, not Fœrst., 1856.
Isoglossa Coq., 1895, not Casey, 1893.
Itamus Loew, 1849, not Schm.-Goeb., 1846.
Latreillia Desv., 1830, not Roux, 1827.
Laverania Theob., 1902, not Grassi & Fel., 1890.
Leptochilus Loew, 1872, not Sauss., 1852.
Leptopus Fall., 1823, not Latr., 1809.
Leptopus Hal., 1831, not Latr., 1809.
Lissa Meig., 1826, not Leach, 1815.
Lophonotus Macq., 1838, not Steph., 1829.
Macrochira Zetters., 1838, not Meig., 1803.
Macrurus Lioy, 1864, not Bonap., 1841.
Meckelia Desv., 1830, not Leuck., 1828.
Microcera Zetters., 1838, not Meig., 1803.
Mochtherus Loew, 1849, not Schm.-Goeb., 1846.
Mycetina Rond., 1856, not Muls., 1846.
Myobia Desv., 1830, not Heyd., 1826.
Odontocera Macq., 1835, not Serv., 1833.
Okenia Zetters., 1838, not Leuck., 1826.
Omalocephala Macq., 1843, not Spin., 1839.
Pales Desv., 1830, not Meig., 1800.
Panoplites Theob., 1900, not Gould, 1853.
Phoneus Macq., 1838, not Kaup, 1829.
Plagiocera Macq., 1842, not Klug, 1834.
Plagiotoma Loew, 1873, not Clap. & Lachm., 1858.
Plectropus Hal., 1831, not Kirby, 1826.
Polydonta Macq., 1850, not Fisch., 1807.
Psilopus Meig., 1824, not Poli, 1795.
Pygostolus Loew, 1866, not Hal., 1833.
Rhopalomyia Willist., 1895, not Rübsaam., 1892.
Rœselia Desv., 1830, not Hübn., 1816.
Rondania Jænn., 1867, not Desv., 1850.
Sargus Fabr., 1798, not Walb, 1792.
Sicus Latr., 1796, not Scop., 1763.
Stenomacra Loew, 1873, not Stal, 1870.
Stictocephala Loew, 1873, not Stal, 1869.
Subula Meig., 1820, not Schum., 1817.
Tetrachæta Brauer & Bergenst., 1894, not Ehrenb., 1844.
Tetrachæta Stein, 1898, not Ehrenb., 1844.
Trichoptera Lioy, 1864, not Meig., 1803.
Triodonta Willist., 1885, not Bory, 1824.
Trupanea Macq., 1838, not Schrank, 1795.
Wulpia Brauer & Bergenst., 1893, not Bigot, 1886.

17. These names are published herewith for the information of all persons interested. They will be forwarded by July 1, 1912, to the International Commission on Zoological Nomenclature, the Commission on Nomenclature of the International Entomological Congress and to several entomological committees and societies.

18. A vote will be called on these names at the next meeting of the International Commission on Zoological Nomenclature, in the summer of 1913, and any objection to the proposed action should be filed with the under-

signed, and stating ground for the objection, not later than May 1, 1913.

C. W. STILES.
Secretary International Commission on Zoological Nomenclature

SPECIAL ARTICLES

ŒNOTHERA NANELLA, HEALTHY AND DISEASED

IN my cultures of the evening primrose of Lamarck, the pure and self-fertilized seed yearly produces a certain percentage of mutants, among them dwarfs, *Œnothera nanella,* the number of which usually amounts to about 1 per cent. of the whole crop. Of late, these dwarfs have been the subject of some discussion, since Zeylstra discovered the presence of a bacterium in their tissues and showed that some of their characters, formerly considered as specific marks, are, in reality, abnormalities caused by this parasite.[1] From this, some authors have erroneously concluded that the dwarfs are no real mutants, but only diseased individuals of the original type.[2]

Zeylstra, however, had pointed out that, under favorable conditions, the sideshoots of the dwarfs may become healthy and lose their abnormal characters; but their height remains the same as in the diseased stems. Hence we may assume that, under still more favorable conditions, the main stems themselves might grow up healthy, while still retaining the dwarfish stature.

About half a century ago, Liebig pointed out that nitrogenous manure is apt to increase the sensitiveness of plants to diseases, whilst phosphate of calcium is one of the best means to diminish this predisposition. Laurent found the same to be true for such diseases as are caused by those common bacteria of the soil, which, under normal conditions, are harmless, but may injure the cultures, whenever the manure is too rich in nitrogenous substances. He studied *Bacillus fluorescens putidus* and *B. coli communis,* both of which destroy the cellwalls by means of their enzymes, even before they themselves reach the cells. The bacterium of *Œnothera nanella* is of a wholly different type, since it is found within the living cells and changes their growth without killing them. Zeylstra provisionally placed it in the group of *Micrococcus.*

From these data it is probable that healthy *O. nanella* might be obtained by giving them less nitrogen and more phosphate of calcium. Unfortunately, however, the nitrogen manure acts as the strongest stimulant, under our climate, to induce them to become annual, and for many reasons it is most desirable to have cultures of annual generations. It is, therefore, necessary to determine the amount of nitrogen and phosphate of calcium which will induce a sufficiently large percentage to become annual, but will not essentially heighten their liability to become diseased.

In the summer of 1911, I made some provisional experiments which show that, by this method, there may be produced almost wholly healthy specimens with the normal stature of the dwarfs. In the first place, I found that every part of the stem, every single leaf and flower, may be normal or diseased, in response to external influences. In the young rosettes of rootleaves the first leaves were formerly always twisted; then came long-stalked normal ones and, after these, the really abnormal leaves with broadened and shortened bases, which often killed the terminal bud before it could make a stem. By giving a large amount of phosphate of calcium, and as little nitrogen as possible, every one of the rootleaves could be grown healthy, with a stalk and a narrow wedge-shaped base. The same was the case with the leaves of the stem, and even with the flowers. The number of the abnormal ones could be brought down to a very few, thereby giving the whole plant the appearance of a healthy condition. All transitions between diseased and normal dwarfs were to be seen in these cultures.

Moreover, I have won beautiful healthy dwarfs by means of a cross from which the other parent was eliminated after the rule of the sesquireciprocal crosses.[3] I pollinated a

[1] H. H. Zeylstra, "Fzn. *Œnothera nanella* de Vries, eine krankhafte Pflanzenart," *Biolog. Centralblatt,* 1911, Bd. XXXI., pp. 129–139.

[2] Sammelreferat by G. Tischler, *Zeitschr. f. ind. Abst.,* 1911, Bd. V., p. 327.

[3] "Ueber doppeltreciproke Bastarde," *Biol. Centralbl.,* 1911, T. 31, pp. 97–104.

dwarf of *O. nanella* × *biennis* with the pollen of an ordinary *O. nanella* and got a culture of *O.* (*nanella* × *biennis*) × *nanella* = *O. nanella* which contained a high percentage of healthy plants. They began flowering when only 20 cm. high, the first flower appearing at a height of 10 cm.; whilst *O. Lamarckiana* reached 1.50 m. before flowering, the first flower opening about 80 cm. above the soil. All their leaves were as narrowly elliptical and as clearly stalked as those of the *Lamarckiana* itself, whilst the flowers were free from those abnormalities which usually accompany the dwarfish stature.

Thus we see that the discovery of Zeylstra, far from diminishing the value of *Œnothera nanella* as a real and (in an experimental way) most useful mutant, has given the means of cultivating it in as healthy a condition as may be required.

Hugo de Vries

Behavior of Spermatozoa in Plasma

The recent article of Loeb and Bancroft[1] and of De Meyer[2] in which their observations upon the behavior of spermatozoa in various sorts of solutions, such as extracts of eggs of the same species (De Meyer, eggs of *Echinus microtuberculatus;* Loeb and Bancroft, eggs of the common fowl), colloids, acids, alkalies, hypo- and hypertonic solutions, egg-albumen, blood serum and Ringer solutions are described open up a most interesting field for investigation. During the past summer while occupying a table at the Marine Biological Laboratory, Woods Hole (for the use of which I am indebted to Professor F. R. Lillie), I attempted to grow spermatozoa of *Arbacia punctulata, Mytilus edulis* and *Modiolus modiolus* in various solutions, some of which being listed above as used by these other workers.

[1] *Journ. Exp. Zoology,* 12: 381.

[2] *Arch. Biol.,* 1911, Bd. 26, H. 1, pp. 65–97: "Observations et expériences relatives a l'action exercée par des extraits d'œufs et d'autres substances sur les spermatozoïdes." I have seen only Robert Lewin's review in the *Zentralb. für Biochemie und Biophysik,* XII., No. 19/20, of De Meyer's paper.

On August 2, I centrifuged *Limulus* blood plasm and made a hanging drop from the upper layer, which examination showed to be free from cells; into this drop I introduced a few sperms from *Arbacia.* Great difficulty was experienced in attacks of bacteria and many of the preparations were discontinued the following morning. The slides were sealed with vaseline, as in the usual culture mount, and left at room temperature. By the eighth of August there was no movement in the sperms, although it had persisted up until that time and therefore the copper component of the blood of this animal does not seem to be toxic for *Arbacia* sperms, but none of the phenomena about to be described from mounts in different media were observed.

On August 5, a culture was made in the sterile agar medium, made according to the customary bacteriological formula, diluted so that it was liquid but highly viscid at 20° C. The spermatozoa lived only a short time and were seen to disintegrate within 24 hours. It may be stated that the reaction of the agar was estimated only roughly by an indicator and not titrated, so that I am not certain whether the medium was suitable from this standpoint. Care was taken to render the sea-urchins as free from bacteria as possible, the tests being washed off with $HgCl_2$, 1:1,000 before the cuts were made and sterile sea-water was used to receive the testes after extirpation. The mounts remained sterile throughout the time of observation, showing that the testes are bacteria-free, as one would suspect.

The plasma of a Norway rat was then tried on August 8 and this was prepared by centrifuging the blood of the rat in paraffin-lined tubes at about 8°–10° C. The plasma clotted when the hanging drop was made at room temperature, but sufficient time elapsed before the plasma clotted for the introduction of the sperm. The behavior of the sperm-heads was discovered to be quite like that described by Loeb and Bancroft for the sperm of the fowl, for the heads enlarged, became less dense, and distinct chromatin granules were visible, even in unstained preparations, resembling the nuclei of the spermatids of certain insects

which I have observed in a living condition without stains, the appearance being in this case checked with stained preparations. The sperms were active and the head and tail wriggled in their characteristic manner as long as they were visible. The tail became shorter and shorter as the head swelled, but in none of my specimens did the tail-cytoplasm completely incorporate itself into the head. This is true, I believe, for Loeb and Bancroft's experiments. In other words, a completely rounded out cell, like a spermatocyte, did not appear in these preparations.

De Meyer succeeded in causing the heads to swell by growing the sperms in a dilute solution of gelatin (gelatin *sol*); every indication pointed to the perfect imitation of the formation of the pronuclear condition in a normally fertilized egg. It is of the greatest interest, too, to observe that the experiments made by De Meyer in acid solutions gave exactly the same result as colloidal solutions in general—that is, a swelling in acid media.

These experiments and those of Loeb and Bancroft show the possibility of approaching the explanation of the behavior of the spermatozoon during fertilization upon physical-chemical grounds. Factors leading to mitosis should be determined and the various artificial parthenogenetic reagents should be tried.

I have recently determined, also, that if a trace of saponin be added to the water in which the spermatozoa of *Cerebratulus* lie, there is a slight cytolysis and swelling of the head of the spermatozoon, but the "tail" is not affected, apparently. Whether mitosis can be induced in this manner, as it can in the egg, in the formation of polar bodies, as I have elsewhere described, remains yet to be determined.[2]

MAX MORSE

TRINITY COLLEGE,
HARTFORD, CONN.,
April 10, 1912

[2] I am under obligation to the officers of the biological laboratories of Yale University, Professors Harrison, Coe, Woodruff and Petrunkevitch, for the opportunity to study living nemertean eggs and sperms.

SOCIETIES AND ACADEMIES

RESEARCH WORKERS IN EXPERIMENTAL BIOLOGY, WASHINGTON, D. C.

AT the meeting of this society, held on February 21, 1912, Dr. William N. Berg, of the Bureau of Animal Industry, gave a critical exposition of Zuntz's theory in regard to the physical-chemical basis of striated muscle contraction,[1] in which it was pointed out that this theory had many objectionable features. These may be summarized briefly as follows:

(*a*) Lymph contains practically no carbon dioxid in the gaseous state.

(*b*) Gases dissolved in water do not behave entirely like true solutes, and exert no osmotic pressure; exceptions are hydrochloric acid, ammonia and a few other gases.

Accordingly, the carbon dioxid produced by muscle contraction can not exert any osmotic pressure, and, furthermore, it is not shown in Zuntz's work that the walls of the muscle rods are impermeable to carbon dioxid during the contraction phase. This is necessary, for otherwise osmotic equilibrium could not be brought about by the inflow of water alone; an outflow of carbon dioxid must take place. A further objection is that carbon dioxid at the moment of its formation does not have a temperature of nearly 6000° C.

At the March meeting, held on the 20th inst., Dr. William Salant, chief of the pharmacological laboratory of the Bureau of Chemistry, gave a brief résumé of the caffein investigations which were conducted in the Department of Agriculture, and which embrace studies on the effects of different amounts of caffein upon the organism, with especial reference to the production of acute and chronic intoxication. Other factors, such as the influence of diet, age, season, etc., were considered.

In conjunction with the tests, which were done with carnivorous and herbivorous animals, the rate of demethylation of caffein and the elimination of caffein in the urine and gastro-intestinal canal were noted under normal and pathological conditions.

In addition to the above, the results of experiments upon the effect of caffein upon the circulation, with particular regard to synergism and the antagonism of other drugs, were reported.

LEWIS W. FETZER

[1] "Die Kraftleistung des Tierkorpers; eine Festrede," Kgl. Landw. Hochschule Berlin, 1908.

THE HELMINTHOLOGICAL SOCIETY OF WASHINGTON

The eleventh regular meeting of the society was held at Mr. Crawley's residence on April 16, 1912, Mr. Crawley acting as host and Dr. Graybill as chairman.

In connection with Dr. Ransom's notes on cysticerci, Dr. Cobb called attention to a remarkable case of hydatid disease in Australia, where a man who had had a considerable part of the liver removed, owing to a hydatid infestation, subsequently returned for operative removal of another hydatid. It was found on operation that the second parasite was in another part of the liver and that there had been a practically complete regeneration of the excised portion of the liver.

The evening was devoted to the exhibition and discussion of apparatus, drawings and specimens by Dr. Cobb.

A set of screens for collecting free-living nematodes was shown, the screens being oblong instead of circular, and therefore easier to pack in a suitcase in field work. Two screens using a detachable silk bolting-cloth, instead of an attached brass-wire screening, have a new fastening device consisting of a string looping around projections from the tins. A wooden container for holding collecting bottles fits inside of the screens and of some collecting trays of the same shape as the screens.

Dr. Cobb remarked that he had secured superior results by using sea water in his sublimate fixing reagents, nematodes staining very much better after such treatment. He suggested that this might be due to traces of various chemicals in the sea water, rather than to an increased solubility of the sublimate used.

In connection with a number of specimens and drawings of nematodes, Dr. Cobb pointed out that there is a possibility that some of the free-living forms will be found to show traces of internal segmentation in the arrangement of the internal organs and their relation to the external markings. In an undescribed genus he noted the association of a strong buccal spear with a weak pharyngeal suction bulb, and surmised that the retrorse annulation in this form served to hold it in a tangle of vegetation so that the buccal spear could be used effectively in the absence of a strong suction bulb to fix the mouth in using the spear. In this genus the buccal spear is lost by the male in an ecdysis.

Dr. Cobb expressed the opinion that the study of nematodes will have to be separated from parasitology or helminthology as covering a field of size and importance equal to that of such subjects as entomology. The nematodes make up an isolated group with a very wide range of morphological structure, life history, habits and geographical distribution. The group includes parasites of vertebrates and invertebrates, parasites of plants on land and in water, free-living forms that eat vegetable matter, some that feed exclusively on diatoms, some that feed on bacteria and some that eat other nematodes. Broadly speaking, the mouth parts show the same general lines of variation that insects show, some being adapted to biting, others to sucking or stinging, and it is possible that the buccal stylet of *Mononchus* serves as a poison fang to benumb the nematodes on which this genus feeds. The life histories and methods of reproduction are very variable and include parthenogenesis. Nematodes occur in the soil and in the ocean in immense numbers and have been found in the polar regions. There are probably as many species as there are of insects, and some of the well-marked genera will probably be found to have as many as 500 species.

Some of the nematodes, such as *Streptogaster*, have an anterior dilatation of the intestine which appears to be morphologically and physiologically a stomach.

Dr. Cobb gave an interesting demonstration of the method of obtaining the decimal nematode formula, showing that it could be obtained with no great effort in about six minutes.

Maurice C. Hall,
Secretary

AMERICAN PHILOSOPHICAL SOCIETY

On April 12th, Dr. Willis F. Manges, Röntgenologist to the Jefferson Medical College Hospital of Philadelphia, read a paper before the American Philosophical Society on the X-rays. He reviewed briefly the history of the discovery and especially noted the great progress in safety and efficiency of the apparatus and methods of to-day as contrasted with the earlier results. By means of the modern methods of protection, X-ray injury is now almost entirely eliminated. He discussed the value of the X-rays in medicine and surgery both in diagnosis and treatment. He pointed out also that interpretation of the Röntgenographs required special skill, which could only be gained by a wide experience or careful training and that there was danger in their indiscriminate use in medico-legal cases because of the difficulties of such interpretation.